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THE MEAN DENSITY OF THE EARTH.

In Preparation.

A TEXT-BOOK OF PHYSICS:

INCLUDING

PROPERTIES OF MATTER, HEAT, SOUND AND LIGHT, MAGNETISM AND ELECTRICITY.

BY

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Phys
Mech.
P.

THE

MEAN DENSITY OF THE EARTH.

AN ESSAY

TO WHICH THE ADAMS PRIZE WAS ADJUDGED IN 1893

IN THE UNIVERSITY OF CAMBRIDGE.

BY

J. H. POYNTING, Sc.D., F.R.S.,

LATE FELLOW OF TRINITY COLLEGE, CAMBRIDGE; PROFESSOR OF PHYSICS, MASON COLLEGE, BIRMINGHAM.

"Fortune him hath enhaunced so in pryde,
That verraily he wend he might atteygne
Unto the sterris upon every syde;
And in a balance weyen ech mounteyne."

WITH ILLUSTRATIONS AND SEVEN FOLDING PLATES.

DATE.....

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PREFACE.

THE subject for the Adams Prize for 1893 was "The Methods of determining the absolute and relative value of gravitation and the mean density of the earth."

This Essay, to which the Prize was adjudged, gives an account of the various experiments, including one by the author, which have been made to determine the mean density of the earth.

The First Part consists of an introductory discussion of the problem and an account of the experiments hitherto made on the subject. Since the Essay was first written I have had access to some of the original papers, of which I had before only seen abstracts or brief notices, and I have therefore been able to make the history of the subject more complete.

The Second Part contains an account of my own experiment which has already appeared in the *Philosophical Transactions of the Royal Society for* 1891. As it is a condition of the award of the Prize that the Essay should be printed, it is necessary to give here the account of the experiment in full as already published.

I have prefixed a bibliography of works and papers containing important or noteworthy contributions to the subject. In this will be found a reference to an experiment by M. Berget, of which the account has been published so recently that it could not be included in the First Part of the Essay.

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BIBLIOGRAPHY.

In this list I have only included papers which contain original matter, or which are of interest in the history of the subject. The titles of many others appearing to contain references to the mean density of the earth will be found in the Bibliography of the Pendulum, in vol. iv. "Mémoires Rélatifs à la Physique."

I have not seen any of these, but from the fact that they are not referred to in the papers describing original work, it is probable that they are unimportant, and I have omitted them.

1. 1687. **NEWTON (SIR ISAAC).** Philosophiæ Naturalis Principia Mathematica. Lib. 3. Phaenomena, and Propositiones I.–X.

Newton here shows that the observed phenomena agree with the supposition of universal gravitation according to the law GM_1M_2/d^2, and he deduces the relative masses and specific gravities of the sun and planets.

In Prop. X. he concludes that the earth may be five or six times as dense as water.

In Prop. VI. he describes the box pendulum experiments in which equal weights of different substances are in turn placed in a box forming the bob of a pendulum. The time of swing being the same in all cases, terrestrial gravitation is proportional to the mass and not selective.

2. **NEWTON (SIR ISAAC).** De Mundi Systemate. (Opera, edited by Horsley, vol. iii. p. 194.)

Translated in vol. iii. of Davis's edition of Motte's version of the "Principia," 1803, p. 21.

In a passage on the attraction of terrestrial masses, Newton calculates the attraction of two spheres 1ft. in diameter, and of the same density as the earth, though there is a curious error in the calculation. He also estimates the plumb-line deflection of a hemispherical mountain, three miles high.

3. 1749. **BOUGUER.** La Figure de la Terre. 4to. Paris, 1749. (Referred to in Todhunter's "Theory of Attractions."

Vol. i. pp. 247–8. ✓ Clarke's "Geodesy," pp. 324–6, and Maskelyne's paper No. 4 below.)

The 7th section gives an account of Bouguer's attempts to determine the plumb-line deflection due to Chimborazo, and the increase of gravity at Quito, due to the table-land on which it is situated.

The first work on the subject, though the results were recognised as quantitatively valueless by the experimenter.

4. 1772. **MASKELYNE (REV. NEVIL, B.D., F.R.S., and Astronomer Royal).** A PROPOSAL FOR MEASURING THE ATTRACTION OF SOME HILL IN THIS KINGDOM BY ASTRONOMICAL OBSERVATIONS. (*Philosophical Transactions*, 1775, p. 495.)

A proposal to use some mountain in England, either Whernside or Helvellyn and Skiddaw.

The reading of this paper resulted in the appointment of a committee, of which Maskelyne was a member, to find a suitable hill, and to carry out the necessary work. The surplus remaining from the grant made by George III. to defray the expenses of the observations of the 1769 Transit of Venus was given to the committee by the King.

5. 1775. **MASKELYNE (REV. N., B.D., F.R.S., and Astronomer Royal).** AN ACCOUNT OF OBSERVATIONS MADE ON THE MOUNTAIN SCHEHALLIEN FOR FINDING ITS ATTRACTION. (*Philosophical Transactions*, 1775, p. 500.)

This paper gives a full account of the astronomical work, carried out by Maskelyne, in this celebrated experiment. The zenith distances of a number of stars were observed at two stations respectively on the north and south sides of the mountain. There is also an account of the survey of the district.

The work of computation was undertaken by Hutton, and described by him in the paper No. 6 below.

6. 1778. **HUTTON (CHARLES, F.R.S.).** AN ACCOUNT OF THE CALCULATIONS MADE FROM THE SURVEY AND MEASURES TAKEN AT SCHEHALLIEN, IN ORDER TO ASCERTAIN THE MEAN DENSITY OF THE EARTH. (*Philosophical Transactions*, 1778, pp. 689–788. Revised in the Author's *Tracts*, vol. ii. p. 64, 1812.)

A very full account of the method of determining the difference of attractions at the north and south stations on Schehallien, in terms of the masses in the district. Value obtained for the mean density 4·5. In the revision of 1812 he gives 4·95.

See also Nos. 9 and 12 below.

7. 1780. **HUTTON (CHARLES, F.R.S.).** CALCULATIONS TO DETERMINE AT WHAT POINT IN THE SIDE OF A HILL ITS ATTRACTION WILL BE THE GREATEST, ETC. (*Philosophical Transactions*, 1780, p. 1.)

The maximum effect at about $\frac{1}{4}$ the altitude.

8. 1798. **CAVENDISH (H., F.R.S.).** Experiments to determine the Density of the Earth. (*Philosophical Transactions*, vol. lxxxviii., 1798, pp. 469–526.)

An account of the celebrated experiment devised by Michell, and carried out by Cavendish, to determine the attraction between lead spheres by means of the torsion balance. Value of the mean density 5.448.

9. 1811. **PLAYFAIR. (J.)** Account of a Lithological Survey of Schehallien, made in order to determine the Specific Gravity of the Rocks composing that Mountain. (*Philosophical Transactions*, 1811, pp. 347–378.)

A careful survey of the disposition and specific gravities of the strata round Schehallien, and an estimate of the correction to be applied to Hutton's result, leading to limits 4·5588 and 4·867 between which the mean density should lie.

10. 1814. **ZACH (BARON DE).** L'Attraction des Montagnes et ses Effets sur les Fils à Plomb, etc., Avignon.

A determination of the deviation of the plumb-line by the mountain Mimet, some miles north of Marseilles, the second station being an island nine miles S.W. of Marseilles. The instrument used was probably inadequate for the purpose, and no attempt was made to deduce the mean density.

11. 1820. **LAPLACE (M. DE).** Sur la densite moyenne de la Terre. (*Ann. de Chem. et de Phys.*, xiv., 1820, pp. 410–417. *Tilloch Phil. Mag.*, lvi. pp. 322–326.)

Only a brief account of the results found by experiment up to the time of writing, and a reference to his own work as showing the heterogeneity of the earth.

12. 1821. **HUTTON (C.).** On the Mean Density of the Earth. (*Philosophical Transactions*, 1821, pp. 276–292.)

A résumé of the Schehallien experiment ; a vindication of his claim to have taken an important part in it ; and a criticism of Cavendish's calculations. The errors he thought he detected were later shown by Baily either to be non-existent or unimportant. (*Mem. Ast. Soc.*, xiv., No. 19 below.) His final value from the Schehallien experiment is given as 4·95.

13. 1824. **CARLINI (FRANCESCO).** Osservazione della lunghezza del pendolo simplice fatte all altezza di millettise sul livello del Mare. (*Milano Effem. Astr.* 1824, pp. 28–40. An account of Carlini's experiments by Sabine, *Quarterly Journal of Science*, ii., 1827, pp. 153–159.)

Carlini found the length of the seconds pendulum at the Hospice on Mount Cenis, 1943 metres above sea-level, to be ·210 mm. greater than it would have been without the mountain at the same altitude and latitude. Assuming a cer-

tain shape and density for the mountain, the value of the earth's mean density is 4·39. Sabine gives the result as ·0076 inches longer than it would have been without the mountain at the same altitude, and deduces a value 4·77. See No. 18, below.

14. **1826–8.** Account of Experiments made at Dolcoath Mine in Cornwall, in 1826 and 1828, for the purpose of determining the Density of the Earth. 8vo, pp. 16, Cambridge, 1828.

Privately printed and issued with a request that it should not be published. Early attempts by the method described in No. 22.

Accounts of these experiments are fully given in

15. **DROBISCH (M. W.).** Ueber die in den Minen von Dolcoath in Cornwall neuerlich angestellten Pendel-Beobachtungen. (*Pogg. Ann.*, x. 1827, pp. 444–456.)

16. **DROBISCH (M. W.)** Ausführlicher Bericht ueber mehrere in den Jahren 1825 und 1826 in den Minen von Dolcoath zur Bestimmung der Mittleren Dichtigkeit der Erde angestellte Pendel-Versuche. (*Pogg. Ann.*, xiv., 1828, pp. 409–426.

17. **1837. REICH (F.)** Versuche über die Mittlere Dichtigkeit der Erde mittelst der Drehwage. 8vo. Freiberg, 1838.

Some account of the experiment is given in a note, "Sur la densité de la Terre," *Comptes Rendus*, v. 1837, pp. 697–700. Referred to in Baily, *Mem. Ast. Soc.* 14, No. 19 below.

A repetition of Cavendish's experiment, with smaller spheres and some changes of method. Result 5·44, afterwards corrected to 5·49.

18. **1841. GIULIO (CARLO IGNAZ).** Sur la détermination de la densité moyenne de la Terre déduite de l'observation du pendule faite a l'Hospice du Mont Cenis, par M. Carlini, en Sept. 1821. (*Torino Mem. Accad.*, iii., 1841, p. 379.)

I have not seen this paper, or any account of its contents, beyond a statement by v. Sterneck that the resulting mean density is 4·950.

19. **1842. BAILY (FRANCIS).** An Account of some Experiments with the Torsion-rod for determining the Mean Density of the Earth. (*Memoirs of the Royal Astronomical Society*, vol. xiv. pp. 1–120 and i–ccxlviii.)

Baily's repetition of Cavendish's experiment. A large number of determinations with different attracted masses. Mean value 5·6747.

20. **1847. HEARN (G. W.).** On the Cause of the Discrepancies observed by Mr. Baily with the Cavendish Apparatus for determining the Mean Density of the Earth. (*Philosophical Transactions*, 1847, pp. 217–230.)

A curious attempt to explain the anomalies in Baily's observations by supposing that there was a magnetic action between the masses. The polarity of the attracting masses was supposed to be reversed on changing their position, so that an attraction on one side became a repulsion on the other, and practically only the variations in the amount of polarity would be detected.

These variations he supposes to be somewhat irregular. The total magnetic action he makes out in some cases to be far larger than the gravitative action. He was evidently unaware of Cavendish's experiments on the action of magnets on the balls, or of Reich's with a cast-iron mass.

21. **1852. REICH (F.).** Neue Versuche mit der Drehwaage. (*Leipzig. Abh. Math. Phys.* i., 1852, pp. 383–430.) Neue Versuche über die Mittlere Dichtigkeit der Erde. (*Pogg. Ann.*, lxxxv., 1852, pp. 189–198; *Phil. Mag.* v. 1853, pp. 154–159.)

After explaining that he has revised his former results, using Baily's method and altering the final result from 5·45 to 5·49, he describes a more extensive series giving result 5·5832. Some interesting experiments were made with attracted masses of bismuth and iron. The former gave nearly the same result, the latter a higher one. But in any case, the magnetic action in the original experiment must be negligible.

22. **1856. AIRY (G. B., Astronomer Royal).** Account of Pendulum-Experiments undertaken in the Harton Colliery for the purpose of determining the Mean Density of the Earth. (*Philosophical Transactions*, 1856, pp. 297–342.)

A full account of the experiments, survey, and calculations for the determination of the ratio of gravity above to gravity a known depth below the surface. Value of the mean density 6·565.

23. **1856. AIRY (G. B.).** Supplement to the "Account of Pendulum-Experiments. . . ." (the paper above). (*Philosophical Transactions*, 1856, pp. 343–355.)

An acount of experiments undertaken at Greenwich to determine the temperature correction for the pendulums, with an addendum by Prof. Stokes in reference to the effect of the earth's rotation and ellipticity.

24. **1856. JAMES (Lieut.-Col. R.E., F.R.S) and CLARKE (Capt. R.E.).** On the Deflection of the Plumb-line at Arthur's Seat and the Mean Specific Gravity of the Earth. (*Philosophical Transactions*, 1856, pp. 591–606.)

In the course of the survey round Edinburgh, a determination of the deflection of the plumb-line by Arthur's Seat was made by means of stellar zenith distances.

The paper contains a discussion of the mode of treating the astronomical observations and a full account of the mode of computing the attraction at each station of the surrounding country by Hutton's method. The value of mean density is 5·316. An inquiry is added into the reason for the general deflection of about 5″ in the neighbourhood of Edinburgh.

A fuller account of the data is given in the "Account of the Observations and Calculations of the Principal Triangulation" of the Ordnance Survey 1858.

25. 1873. **CORNU** (A.) ET **BAILLE** (J.). DÉTERMINATION NOUVELLE DE LA CONSTANTE DE L'ATTRACTION ET DE LA DENSITÉ MOYENNE DE LA TERRE. (*Comptes Rendus*, vol. lxxvi., 1873, pp. 954–8.)

A brief account of the apparatus used in a new determination by the torsion balance method and a statement of the mean results obtained so far, viz., 5·50 and 5·56.

26. 1878. **CORNU** ET **BAILLE**. ÉTUDE DE LA RÉSISTANCE DE L'AIR DANS LA BALANCE DE TORSION. (*Comptes Rendus*, vol. lxxxvi., 1878, pp. 571–4.)

An account of some observations to determine whether the resistance of the air is accurately proportional to the velocity. If so, the successive swings should be in exact geometric progression and so they were found to be with great accuracy.

27. 1878. **CORNU** ET **BAILLE**. SUR LA MESURE DE LA DENSITÉ MOYENNE DE LA TERRE. (*Comptes Rendus*, vol. lxxxvi. pp. 699–702.)

A notice of addition of two more spheres of mercury and a reduction of distance from the torsion rod, the force being thus quadrupled. The time of vibration has remained at 408 seconds for more than a year. The results are still 5·56.

An error is pointed out in Baily's method in which he takes the last elongation in one postion as the first in the next, notwithstanding the movement of the masses meanwhile. Rejecting the first elongation in each position as vitiated, Baily's results are appreciably reduced, and the estimate, founded on some random examples, is that the mean would be lowered from 5·68 to 5·55.

28. 1878. **CORNU** ET **BAILLE**. INFLUENCE DES TERMES PROPORTIONNELS AU CARRÉ DES ÉCARTS DANS LE MOUVEMENT OSCILLATOIRE DE LA BALANCE DE TORSION. (*Comptes Rendus*, vol. lxxxvi. pp. 1001–4.)

An examination of the effect of terms higher than the first in the restoring force.

Taking $\ddot{\omega} + h\dot{\omega} + s\omega = W\alpha^2$

and neglecting h^2

$$\omega = A e^{-\alpha t} \sin \frac{2\pi(t - t_0)}{T} + \frac{W A^2 e^{-2\alpha t}}{3s} \left\{1 + \cos^2 \frac{2\pi(t - t_0)}{T}\right\}$$

where $\alpha = \frac{h}{2}$ and $\frac{2\pi}{T} = \sqrt{s - \alpha^2}$

Hence, a term of half the period is superposed on the solution obtained by neglecting $W\omega^2$.

It follows that neither the point through which the rod passes at equal intervals of time nor that about which the successive vibrations have the same ratio is the true position of rest.

29. 1878. **JOLLY** (Ph. von). Die Anwendung der Waage auf Probleme der Gravitation. (*Abhand. der k. Bayer. Akad. der Wiss.*, 2 cl. xiii. Bd. 1 Abth. pp. 157–176; *Wied. Ann.*, vol. v. pp. 112–134.)

Account of experiments made with a balance to determine the variation of weight with height. A kilogramme was weighed in one pan of a balance and then in a pan suspended by a wire from the first and about 5 metres below it. An increase, nearly that calculated by the ordinary theory, was observed. The author suggests that if the experiment were repeated under suitable conditions with a large mass of lead under the lower position, the increase in the gain of weight would measure the attraction of the lead and so give the mean density of the earth.

30. 1878. **POYNTING** (J. H.). On a method of using the Balance with great delicacy, and on its employment to determine the Mean Density of the Earth. (*Proceedings of the Royal Society*, vol. xxviii., 1878, pp. 2–35.)

An account of preliminary experiments to test a method of determining the mean density by putting a lead sphere 1 foot in diameter under the weight hung by a wire from one arm of a balance, The increase in weight gave the attraction and thence the mean density was calculated to be 5·69, but with large deviations from the mean.

31. 1880. **MENDENHALL** (T. C.). Determination of the Acceleration due to the Force of Gravity at Tokio, Japan. (*American Journal of Science*, vol. xx. pp. 124–132.)

Contains an account of the method used in the experiment described below to determine the time of vibration of the pendulum.

32. 1881. **MENDENHALL** (T. C.). On a Determination of

THE FORCE OF GRAVITY AT THE SUMMIT OF FUJIYAMA, JAPAN. (*American Journal of Science*, vol. xxi. pp. 99–103.)

A determination of the relative values of gravity at Tokio and on the summit of Fujiyama, which is a very symmetrically shaped cone. The mean density deduced 5·77.

33. 1881. **JOLLY** (Ph. v.). DIE ANWENDUNG DER WAAGE AUF PROBLEME DER GRAVITATION. Zweite Abhandlung. (*Abh. d. k. Bayer. Akad. d. Wiss.*, 2 cl. 14 Bd. 2 Abth. pp. 26. *Wied. Ann.*, xiv. 1881, pp. 331–355.)

Experiments carried out in accordance with the suggestion at the close of the previous paper. A balance was fixed at the top of a tower. From its scale pans were hung two other scale-pans by wires 21 metres below. The gain in weight of a glass vessel filled with 5 kg. of mercury when moved from the upper to the lower pan on one side of the balance was measured, and then, a large lead sphere a metre in diameter was built up under the lower sphere, and a gain greater by ½ mgm. was observed. From this the mean density of the earth was calculated to be 5·692.

34. 1882. **STERNECK** (R. von). UNTERSUCHUNGEN ÜBER DIE SCHWERE IM INNERN DER ERDE, AUSGEFÜHRT IM JAHRE 1882 IN DEM 1000 METER TIEFEN ADALBERT-SCHACHTE DES SILBERBERG-WERKES ZU PRIBRAM IN BÖHMEN. (*Mittheilungen des k. u. k. Militar. geogr. Inst. zu Wien*, ii., 1882, pp. 77–120.)

An invariable half-second pendulum was swung at the surface and at two different depths in the mine, being compared with a half-second clock rated from a standard clock at the surface by means of a pocket chronometer. The value of gravity at the underground stations, one 456·5 metres below the other, was practically the same. The values for the mean density given by comparison of each lower station with the surface are 6·28 and 5·01.

35. 1883. **STERNECK** (R. von). WIEDERHOLUNG DER UNTER-SUCHUNGEN ÜBER DIE SCHWERE IM INNERN DER ERDE, AUSGEFÜHRT IM JAHRE 1883 IN DEM 1000 METER TIEFEN ADALBERTSCHACTE DES SILBERBERGWERKES ZU PRIBRAM IN BÖHMEN. (*Mitth. des k. u. k. Mil. geogr. Inst. zu Wien*, iii., 1883, pp. 59–94.)

A repetition of the experiments described in the previous paper, with the addition of two more underground stations and greatly improved pendulum observations. Two invariable half-second pendulums were used, one at the surface, the other at one of the four stations at different depths in the mine, the pendulums being interchanged at each pair of stations to complete a determination. The coincidences were observed at the same time at the two stations and in comparison with the same clock, which gave half-second electric signals at the two stations. The mean result for the density of the earth is 5·68, but the value increases with the depth of the lower station.

36. 1883. **BAILLE** (J. B.). Études sur la résistance de l'air dans les mouvements oscillatoires très lents. (*Comptes Rendus*, 1883, vol. xcvi. pp. 1493–5.)

A study by the torsion balance of the variations of resistance with form and dimensions of the moving body and with temperature and pressure of the air.

The resistance per unit area diminishes with increase of surface normal to the motion; it varies with the dimensions and shape of the case, and also varies with temperature and pressure, especially at high pressure. At fractions of an atmosphere the variation is small.

37. 1884. **KÖNIG** (A.) and **RICHARZ** (F.). Eine neue Methode zur Bestimmung der Gravitations-constante. (*Sitzungsberichte der Berl. Akad.*, 1884, p. 1203. *Wied. Ann.*, xxiv., 1885, p. 664. *Nature*, vol. xxxi. p. 484.)

In the last-named place is an account of the proposed method, forwarded by the authors to correct the incomplete description in early notices in the same volume.

In principle the experiment which is now (1892) in progress may be regarded as a development of Jolly's experiment. A balance is mounted on a large block of lead with perforations below the scale pans. The right weight is first above and the left weight below the block; then the right weight is below and the left above. The difference between the two weighings, when corrected for variation in altitude, is four times the attraction of the block.

38. 1884. **HICKS** (W. M.). On some Irregularities in the Values of the Mean Density of the Earth, as determined by Baily. (*Proc. of Cambridge Phil. Soc.*, vol. v. part 2, 1884, pp. 156–161.)

The author points out that if Baily's results be arranged in order of temperature there is a steady fall in value of the mean density with rise of temperature.

39. 1885. **STERNECK** (R. von). Untersuchungen über die Schwere im Innern der Erde, ausgeführt im Jahre 1885 in dem Abrahamschacte des Silberbergwerkes "Himmelfahrtsfundgrube" bei Freiberg in Sachsen. (*Mitth. des k. u. k. Mil. geog. Inst. zu Wien*, vi. 23 pp.)

Professor Bruhns had found a decrease in gravity in this mine below the surface. Major von Sterneck, therefore, determined to reinvestigate the value with his invariable pendulums, using the two underground stations selected by Bruhns and two others. He found an increase of value with increase of depth but much more rapid than at Pribram. The increase with increase of temperature was, however, nearly the same in both cases, a result at present unexplained. The values of the mean density of the earth were much higher than before, the mean being over 7.

40. 1885. **WILSING** (J.). Bestimmung der Mittleren Dichtigkeit der Erde mit Hülfe eines Pendel-apparates. (*Publica-*

tionen des Astro-Physikalischen Observatoriums zu Potsdam, No. 22, vi. Band, 2 Stück, pp. 35–127.)

A pendulum rod, with nearly equal balls at the upper and lower ends, is supported by a knife-edge just above its centre of gravity so that the gravity restoring couple is small and the time of swing great. Attracting masses are brought up one on one side of the upper ball, the other on the other side of the lower ball, and the deflection is observed. From the time of swing the attraction can be calculated, and the mean density of the earth is deduced as 5·594.

41. 1889. **WILSING** (J.). Bestimmung der Mittleren Dichtigkeit der Erde mit Hülfe eines Pendel-apparates (Zweite Abhandlung). (*Publ. des Astro.-Phys. Observ. zu Potsdam*, No. 23, vi. Band, 3 Stück, pp. 133–191.)

An account of further experiments by the pendulum balance described in the previous paper. Various improvements are introduced, especially some to guard against temperature changes.

The result is now 5·579.

42. 1889. **BOYS** (C. V., F.R.S.). On the Cavendish Experiment. (*Proceedings of the Royal Society*, vol. xlvi. pp. 253–268.)

A discussion of the best dimensions for the torsion apparatus and a description of a small instrument which, with half-inch beam and small attracting masses, gives a large deflection when the period of vibration is only 80 seconds. The author states his intention to construct apparatus for absolute measurements of the gravitation constant.

43. 1889. **LASKA** (W.). Ueber einen neuen Apparat zur Bestimmung der Erddichte. (*Zeitschrift für Instrumenten Kunde*, 1889, vol. ix. pp. 354–5.)

An account of apparatus prepared to carry out a pendulum-deflection method after the method of Wilsing, the position of the pendulum being indicated by interference bands between a glass plate on its upper end and a nearly parallel fixed plate, after the manner of Fizeau's expansion apparatus. The attracting mass to be a sphere of mercury.

No results given.

44. 1892. **POYNTING** (J. H.). On a Determination of the Mean Density of the Earth and the Gravitation Constant by Means of the Common Balance. (*Philosophical Transactions*, vol. clxxxii. (1891), A, pp. 565–656.)

The Paper is reprinted as the Second Part of this Essay.

45. 1893. **BERGET** (A.). Détermination expérimentale de la constante de l'attraction universelle, ainsi que de la

MASSE ET DE LA DENSITÉ DE LA TERRE. (*Comptes Rendus*, vol. cxvi. pp. 1501–3.)

Above the surface of a lake 32 hectares (79 acres) in area, at Habay-la-Neuve, in Belgian Luxemburg, was fixed a hydrogen gravimeter, or a barometer with the cistern closed and containing hydrogen to act as a spring to support the column of mercury. The level of the lake could be varied, and as it fell the mercury in the gravimeter became lighter, and the hydrogen supported a longer column. The position of the column was determined by interference fringes, formed between the surface of the mercury in the vacuum and the end of the tube which was an optically worked plane. The level of the lake was lowered in two stages of 50 cm. each, and then raised again in equal stages. The column of mercury rose $63/10^8$ cm. and $126/10^8$ cm., and then fell equal amounts within 2%. From these observations the mean density of the earth is calculated to be 5·41. In *Comptes Rendus*, vol. cxvii. pp. 96–7, M. Gouy points out that the accuracy claimed implies a temperature constant within a five-millionth of a degree during the several hours occupied by the experiment, a constancy utterly beyond anything as yet considered possible.

The two following papers appear to contain accounts of early attempts to measure variation of gravity with variation of distance from the earth's centre. I have not seen the papers, and know nothing as to their quality, though the result obtained by Bertier seems to indicate a want of exactness.

1773. **BERTIER (Le Père J. C.).** LETTRE DU P. BERTIER DE L'ORATOIRE, OÙ IL EXPOSE UNE EXPÉRIENCE QUI TEND À PROUVER QUE LES CORPS PESENT D'AUTANT PLUS QU'ILS SONT PLUS ÉLÉVES SUR LA TERRE JUSQU'À UNE PETITE DISTANCE. (*Journal de Physique*, ii., 1773, pp. 251 and 275.)

WOLF (*Mémoires rél. à la Phys.*, 4. B. 55) says that this experiment was the same as Jolly's first experiment. Other letters and memoirs on the subject are in vol. v. of the *Journal de Physique*.

1775. **DOLOMIEU (De).** EXPÉRIENCES SUR LA PESANTEUR DES CORPS À DIFFÉRENTES DISTANCES DU CENTRE DE LA TERRE, FAITES AUX MINES DE MONTRELAY EN BRETAGNE. (*Journal de Physique*, 6 Juillet 1775.)

THE MEAN DENSITY OF THE EARTH.

PART I.

HISTORICAL.

INTRODUCTORY DISCUSSION OF THE NATURE OF THE INVESTIGATION.

THE determination of the mean density of the earth may be regarded also as the determination of the complete expression of Newton's law of gravitation. A brief consideration of the facts set forth by Newton in the third book of the "Principia" will show this most clearly, and will at the same time suggest modes in which the determination should be made.

Starting with any single planet—say the earth—and referring its position to a system, fixed relative to the sun and the distant stars, direct astronomical observation shows that it may be described to a close approximation, as moving in an ellipse with the sun in one focus and at such speed that the line from the centre of the sun to the centre of the planet sweeps out equal areas in equal times. This implies, as Newton showed, that the acceleration of the planet is towards the sun and inversely as the square of its distance from that body.

Now comparing the different planets, observation shows that $(\text{length of year})^2/(\text{mean distance})^3$ is the same for each, and from this it follows that the constant of acceleration is the same for all, or that at the unit distance they would all have the same acceleration if the law holding for each in its own orbit held for it at all distances.

So far this is mere time-geometry, or a description of position and rate of change of position, and we might have other equally true, if less convenient, modes of description referred to other standards, such as the epicyclic geocentric mode of the ancients, or

the practical mode in common use in which the co-ordinates of a planet are measured with regard to some observatory, its meridian, and horizon.

But if we regard the accelerations as indicating forces the different methods of description are no longer equivalent. We must select that which gives a system of forces most consistent in itself and most in accord with our terrestrial experience. Here the heliocentric method, with the modification described hereafter, is immensely superior to any other, and, adopting it, we must suppose that the accelerations of the planets indicate forces towards the sun, and since the constant of acceleration is the same for all, that the forces on equal masses are inversely as their distances squared from the sun, whatever planets the masses belong to. In other words, the sun has no favourite among its attendants, but pulls on each pound of each according to the same rule.

But the assumption that the accelerations indicate forces of the kind we experience on the earth, carries with it the supposition of equality of action and reaction, and so we conclude that each planet reacts on the sun with a force equal and opposite to that exerted by the sun on the planet. Hence, each acts with a force proportional to its own mass, and inversely as the square of its distance away. If we suppose that there is nothing special in the attraction of the sun beyond great magnitude corresponding to great mass, we must conclude that the sun also acts with a force proportional to its mass. But we have just shown that the force is proportional to the mass acted on. Hence, we have the force proportional to mass of sun × mass of planet / (distance apart)^2.

Now, turning to any of the smaller systems consisting of a primary and its satellites, the shape of orbit and the motion of the satellites agree with the supposition that the primary is acting with a force according to the inverse square law. It is important for our special problem to note here that in the case of the earth we must include in the term satellite any body at its surface which can be weighed or moved.

We are therefore led to conclude that the law is general, or that if we have any two bodies, of masses M_1 and M_2, d apart, the force on either is

$$\frac{G\,M_1\,M_2}{d^2},$$

where G is a constant, the constant of gravitation.

The acceleration of one of them, say M_2, towards the other is

$$\frac{G\,M_1}{d^2}.$$

If this conclusion is accepted, we can at once determine the masses of the various primaries in terms of that of the sun for

acceleration of satellite towards primary

$$= G.\ \text{Mass of primary} / (\text{distance of satellite})^2$$

and acceleration of primary towards sun

$$= G.\ \text{Mass of sun} / (\text{distance of primary})^2.$$

By division G is eliminated, and we obtain the ratio of the masses in terms of quantities which may be measured by observation.

A confirmation of the generality of the law is obtained from the perturbations of the planets from the elliptic orbits which we have for simplicity supposed them to describe.

These perturbations, in any one planet, can at least approximately be analysed into separate disturbances, each due to one of its fellow planets, acting with a force inversely as the square of its distance away, and if we assume this force proportional to the mass of the disturber we obtain another measure for this mass in terms of that of the sun.

The concordance of the two methods is as complete as we could expect.

The determination of the masses of the different members of our system in terms of that of the sun, enables us to choose a still more satisfactory origin for our system of reference than the centre of the sun—viz., the centre of mass of the whole system. The change is small, but without it we could not account for all the motions merely by a set of inverse square forces in which action and reaction were equal and opposite.

We have for simplicity considered the sun and planets as without appreciable dimensions as compared with their distances apart. But measurement shows that they are all approximately spheres, and the attraction on a sphere with density varying only with the distance from the centre, *i.e.*, consisting of homogeneous concentric shells, if it is considered as the resultant of the attractions on the separate particles, all according to the same inverse square law, is the same as that on the whole mass collected at the centre of the sphere. Further, if the attraction is due, not to the attracting body as a whole but to its separate parts, each acting, as it were, independently and according to the same law, then an attracting sphere acts as if it were all concentrated at its centre. Since the planets, to a near approximation, behave as if they were merely concentrated masses at their centres, and since the deviations from this behaviour, such as the earth's precession, can all be accounted for by their departure from sphericity, we have strong presumption that the attraction is really the resultant of all the attractions, each element m_1 of one body acting on each element m_2 of the other with force Gm_1m_2/d^2.

Astronomical observation enables us, then, to compare the masses

of the various members of the solar system, with each other or even to make a table of specific gravities, choosing any one as the standard substance. Thus, if we take the earth as standard, the mean specific gravity of the sun is about 0·25, that of Mercury about 1·25, that of Venus and Mars about 0·9, and so on.

But this does not give us any idea of the specific gravity in terms of known terrestrial substances or any idea of the masses in terms of the terrestrial standards, the kilogramme or the pound. It is true that Newton, with little more than the astronomical data at his command, made a celebrated guess on the specific gravity of the earth in terms of water, which runs thus in Motte's translation of the "Principia" (vol. ii. p. 230, ed. 1729, book iii. prop. x.): "But that our globe of earth is of greater density than it would be if the whole consisted of water only, I thus make out. If the whole consisted of water only, whatever was of less density than water, because of its less specific gravity, would emerge and float above. And upon this account, if a globe of terrestrial matter, covered on all sides with water, was less dense than water, it would emerge somewhere; and the subsiding water falling back, would be gathered to the opposite side. And such is the condition of our Earth, which, in great measure, is covered with seas. The Earth, if it was not for its greater density, would emerge from the seas, and according to its degree of levity, would be raised more or less above their surface, the water and the seas flowing backwards to the opposite side. By the same argument, the spots of the sun which float upon the lucid matter thereof, are lighter than that matter. And however the Planets have been form'd while they were yet in fluid masses, all the heavier matter subsided to the centre. Since, therefore, the common matter of our Earth on the surface thereof, is about twice as heavy as water, and a little lower, in mines is found about three or four, or even five times more heavy; it is probable that the quantity of the whole matter of the Earth may be five or six times greater than if it consisted all of water, especially since I have before shewed that the Earth is about four times more dense than Jupiter."

It is not a little remarkable that Newton hit upon the limits between which the values found by subsequent researches have nearly all laid.

In order, then, to complete the expression of the law of gravitation, we must connect the celestial with the terrestrial scale of densities. In fact, we must do for the masses of the solar system that which we do for their distances in the determination of the solar parallax, though we cannot proceed quite so directly in the former case as in the latter in connecting the celestial and terrestrial measures. If we could measure the acceleration, say, of the moon, due to any terrestrial body of known shape and density; if, for instance, we knew the form and extent of our tidal wave and

its full lunar effect, we could at once find the mass of the earth in terms of that of the wave, or its density as compared with sea-water.

But at present this cannot be done with any approach to accuracy, and the only method of solving the problem consists in finding the attraction between two bodies on the earth of known masses a known distance apart, and comparing this with the attraction of the earth on a known mass at its surface instead of its attraction as a heavenly body. Since the law of attraction is by observation the same at the surface of the earth and at a distance, we can thus find the mass of the earth in terms of either of the known masses.

To take an illustration from an experiment hereafter described, let us suppose that a spherical mass of 20 kilos. is attracted by another spherical mass of 150 kilos. when the centres are 30 cm. apart with a force equal to the weight of $\frac{1}{4}$ mgm. or $\frac{1}{8000000}$ of the weight of the 20 kilos when the latter is on the surface of the earth and 6×10^8 cm. from its centre, we have:

$$\frac{\text{Mass of Earth}}{(6 \times 10^8)^2} : \frac{150{,}000}{30^2} = 1 : \frac{1}{8000000}$$

whence mass of earth $= 5 \times 10^{27}$ grammes nearly.

The volume of the earth is about 9×10^{26} cc., whence the mean density of the earth is about 5·5.

Or, using the experiment to give the constant of attraction, and expressing the masses in grammes, the weight of $\frac{1}{4}$ mgm. or $\cdot 00025g = \dfrac{G \times 150{,}000 \times 20{,}000}{30^2}$

Whence if $g = 981$,

$$G = \frac{981 \times \cdot 00025 \times 30^2}{150{,}000 \times 20{,}000}$$

$$= \frac{7}{10^8} \text{ nearly.}$$

A determination of G completes the expression of the law of gravitation.

The Methods of Experiment.

These naturally fall into two classes. In the one class some natural mass is selected, either a mountain or a part of the earth's crust, and its mass and form are more or less accurately determined by surveys and mineralogical examination. Its attraction on a plumb-bob at one side, or on a pendulum above or below it is then

compared with the attraction of the whole earth on the same body.

In the other, the laboratory class of experiment, a smaller mass, such as may be easily handled, is placed so as to attract some small suspended body, and this attraction is measured and compared with the attraction of the earth for the same body.

Suppositions as to the Qualities of Gravitation.

In all the methods certain suppositions are made as to the qualities of gravitation, which are probably justified either by the results of the experiments themselves or by other experience.

The following is a list of these suppositions with their justifications:

1. That the law of gravitation holds throughout the long range from interplanetary distances down to the distances between the attracting bodies in the laboratory experiments.

The first step in the descent from celestial spaces is justified by the fact that the acceleration of gravity at the earth's surface agrees with its value on the moon, as attracted by the earth. The further step downward appears to be justified by the fair agreement of the results obtained by the various forms of Cavendish, balance, and pendulum experiments on the mean density—experiments which have been conducted at distances varying from feet down to inches. Where the law ceases to hold is yet a matter for experiment to determine. When bodies come into what we term contact, the adhesion may possibly still be due to gravitation, according to the inverse square law, though the varying nature of the adhesion in different cases seems to point to a change in the law at such minute distances.

2. That gravitation is not selective like electric and magnetic action.

The agreement of astronomical observations with deductions from the law is hardly a sufficient proof of this supposition. For suppose that some matter were attracted more than in proportion to its mass and some less, there might be in all the planets such quantities of the two kinds that the same general average is maintained.

This may be an extravagant supposition, and yet, if we are to adopt some of the current ideas regarding comets and their tails, we are getting very near it. But with regard to ordinary terrestrial matter, Newton's hollow pendulum experiments ("Principia," lib. iii. prop. vi.) repeated with more detail and precision by Bessel ("Versuche über die Kraft, mit welcher die Erde Körper von verschiedene Beschaffenheit anzieht," Abhand. der Berl. Ak. 1830, p. 41; or "Mémoires relatifs à la Physique," tome v. pp. 72–133)

are conclusive that the earth as a whole is not selective. Even here, however, the result might be due to an average of equal excesses and defects. But again we may quote the various mean density experiments and especially those made by Bailey, in which a number of different attracting and attracted substances have been used with nearly the same results.

3. There is no varying quality in the medium intervening between the attracting masses corresponding to electric specific inductive capacity or magnetic permeability.

Astronomical observations are not decisive, for the medium intervening between the sun and planets, approaches a vacuum where so far we have no evidence for variation in quality even for electric and magnetic induction. In the case of the earth too, its spherical form might render observation inconclusive, for just as a sphere composed of concentric dielectric shells each with its surface uniformly electrified would have the same external field in air, whatever the dielectric constant, if the quantity of electrification within were the same, so the earth might have the same field in air whatever the varying quality of the underlying strata as regards the transmission of the action across them if they were only suitably arranged.

But the fact that there is nothing like refraction of the lines of gravitative force corresponding to the tangent law of refraction for electric and magnetic lines of induction appears decisive. If further evidence be needed it may be pointed out that again the different mean density experiments come to our aid. Various forms of case of various material have been used to separate the attracting and attracted bodies, with no great effect on the result.

Further, different forms of attracting masses have been used, the cylinders of Dr. Wilsing giving nearly the same result as the spheres of other workers. The field due to a cylinder would almost certainly depend on its gravitative permeability.

4. There is nothing of a directive nature in the gravitative field.

It is possible to imagine that, superposed on the ordinary field, there is a directive one, which might manifest itself in certain cases. Thus, an elongated body might tend to place its longer axis vertical on the earth's surface. Nothing of the kind has, however, been detected, and for ordinary bodies we may dismiss the supposition. But there is one case which appears to require further experimental investigation, that of crystalline structure with different properties along different axes. Thus, the attraction between two crystal spheres might be different for a given distance according as their like axes were parallel or crossed.

If such difference existed it should show itself by a directive action on one sphere in the field of the other, For allow one

sphere to approach the other with the axes arranged in such way that there is a maximum pull, then revolve the sphere brought up into the position of minimum pull, then withdraw it. A surplus of work remains over unless work has been done in the revolution, *i.e.*, unless there is a couple tending to keep the sphere with its axes in the position of maximum pull. We can hardly imagine any other way in which the surplus could disappear, so that we must suppose a directive action if there is a difference of pull for different positions of the axes.

I propose to investigate this experimentally by means of two quartz spheres, respectively about 1 and $2\frac{1}{2}$ inches diameter. The smaller one will be suspended by a quartz fibre, but as its weight (25 grms.) would require a rather strong fibre to support it, it will be suspended in water and so nearly buoyed up by a sort of small Nicholson's hydrometer that it only just sinks. To this the fibre will be attached; the fibre will then only need to be strong enough to bear the difference between the weight of the sphere and the upward pull of the buoy. A mirror attached to the buoy will indicate in the usual way, by telescope and scale, the azimuth of the sphere.

Outside the water vessel the larger sphere will be placed and turned about its centre in different directions.

I have already made some experiments, without success, to test the existence of a difference of direct pull for different axial directions by suspending the small sphere and buoy at the end of a torsion arm, but the small torsion of the fibre was quite unable to keep the rod from turning round, under the disturbing action of convection currents. The buoy, sooner or later, came against the side of the vessel and here the adhesion prevented return. While planning a reconstruction of the apparatus, it occurred to me that there should be a directive couple, at least of the order of the difference of the moments of the direct pulls at the end of the torsion rod, and a rough calculation on assumed data confirms this. I am now arranging the apparatus to test its existence.*

But even should such action exist, it would hardly affect the conclusions drawn from mean density experiments, for in these there has been at least no regularity of crystalline structure, and in some cases, as in the mercury used by Cornu and Baille no question of crystalline structure at all.

5. That the density obtained from the formula used to express the attraction of the earth is the real mean density, *i.e.* $\frac{\int \rho\, dv}{\int dv}$.

In the calculation of the mean density the earth is replaced by

* Since the above was written the suspension in a liquid by the aid of a buoy has been abandoned, for the convection currents, even with the central position of the mass, produced enormous disturbances. The apparatus is now being prepared with a much smaller suspended crystal in air.

an ellipsoid of the same form as nearly as measurement can determine it, and of uniform density, and the result actually obtained is the value of such uniform density, as would produce the observed value of gravity. But though the density of the earth undoubtedly varies, increasing at least at first as we approach the centre, "it is probable that the earth consists approximately of spherical strata of equal density. Any material deviation from this arrangement could hardly fail to produce an irregularity in the variation of gravity and consequently in the form of the surface, since we know that the surface is one of equilibrium. Hence, we may assume, when not directly considering the ellipticity, that the density ρ is a function of the distance from the centre." (Stokes' "Math. Papers," vol. ii. p. 122.) With this assumption the masses of the actual and of the hypothetical uniform earth must be the same, and the uniform density of the latter must be the mean density of the former. And indeed without any assumption the fact that gravity at the earth's surface and at the moon may both be expressed in terms of the same uniformly dense spheroid and the distances, shows that the two densities are the same, for the actual and the hypothetical earth must have the same mass to have the same attraction at the distance of the moon.

HISTORICAL ACCOUNT.

Newton's Calculations of the Attraction between Terrestrial Bodies.

From a passage in his treatise "De Mundi Systemate" (*Opera*, ed. Horsely, vol. iii. p. 194, § 22), it is evident that Newton considered the possibility of measuring the attraction between terrestrial masses, for he says: "The experiments in terrestrial bodies come to no account; for the attraction of homogeneous spheres near their surfaces are as their diameters. Whence, a sphere of one foot in diameter, and of a like nature to the earth, would attract a small body placed near its surface with a force 20,000,000 times less than the earth would do if placed near its surface; but so small a force could produce no sensible effect. If two such spheres were distant but by $\frac{1}{4}$ of an inch, they would not, even in spaces void of resistance, come together by the force of their mutual attraction in less than a month's time; and less spheres will come together at a rate yet slower—viz., in the proportion of their diameters. Nay, whole mountains will not be sufficient to produce any sensible effect. A mountain of a hemispherical figure, three miles high and six broad, will not, by its attraction, draw the pendulum two minutes out of the true

perpendicular" (Davis's edition of Motte's translation of the "Principia," etc., 1803, vol. iii. p. 22). Unfortunately, there are two errors in the earlier part of the quotation, one of them leading to an enormous under-estimate of the effect of the attraction of spheres on each other. In the first place, the diameter of the earth is about 40,000,000 feet, so that the pull of two spheres on each other, one foot diameter each, one foot from centre to centre, and of the earth's mean density, would be $\frac{1}{4} \times \frac{1}{40000000}$ of the weight of either. Secondly, the acceleration towards each other would be this fraction of 32 feet / sec.^{2} or of 384 inches / sec.^{2}, and the time taken to move $\frac{1}{8}$ inch would be given by $\frac{1}{8} = \frac{1}{2} t^2 \times \frac{384}{160000000}$, whence $t = 320$ seconds, about, instead of a month. Had Newton realised this, he would hardly have dismissed so summarily the possibility of observing the attraction. Still, the passage deserves attention as showing that Newton fully recognised the two methods of experiment by which any effect should be sought for, the only two methods which have yet been discovered.

The Experiments of Bouguer in Peru.

(*No.* 3 *in the Bibliography.*)

The honour of making the first experiments on the attraction of terrestrial masses is to be accorded to Bouguer, a member of the celebrated commission sent in 1735 by the French Academy of Sciences to measure an arc of the meridian in Peru. He attempted, both by pendulum and by plumb-line experiments, to prove the existence, and to measure the attraction, of mountain masses, and the seventh section of his "Figure de la Terre," entitled "Détail des Expériences ou Observations sur la Gravitation, avec des remarques sur les causes de la Figure de la Terre," contains a full account of the work. These experiments cannot be regarded as having any quantitative value. But, as being the first made, as pointing out the methods, and as certainly showing the existence of the attraction sought, they are important in the history of the subject. Regarding them as preliminary trials, I shall give an account of the plumb-line and pendulum experiments together, instead of dealing with them under the separate classes with the later experiments.

The Pendulum Experiment.—Bouguer determined the length of the seconds pendulum at three elevations: (1) at Quito, which may be regarded as on a table-land, the station being 1466 toises above sea-level; (2) on the summit of Pichincha, a mountain rising above Quito to a height of 2434 toises above sea-level; and (3) on the island of Inca in the river Esmeralda, not more than 30 or 40 toises above sea-level. This station is to the north-west of

Quito, and about half a degree north of that town in latitude, the equator lying between them. The toise being taken as 6·395 feet, we may put the elevations in descending order as 15,600 feet, 9,400 feet, and 0. The Quito observations were made at various times: those on Pichincha in 1737, and those at the Isle of Inca in 1740.

The pendulum consisted of a small double truncated cone of copper suspended from fixed jaws by an aloe fibre. The thread was always made of the same length as a certain iron measuring rod, so that the pendulum was "invariable." The time of vibration was found from the number of oscillations gained in a given time on a seconds clock rated by astronomical observations. The temperature of Quito was regarded as equal to that of Paris in the middle of spring, and the measuring rod was taken as correct there. It was therefore necessary to apply temperature corrections to the lengths obtained at the higher and lower levels. Though thermometers were used, the corrections do not appear to have been more than approximate estimates. The buoyancy of the air was also allowed for, its value on Pichincha being determined as $\frac{1}{11000}$ of the weight of the copper bob by noticing how far the barometer must be carried down hill to rise one line.

The following table gives the results:

Station.	Above Sea Level. In Toises.	Observed Seconds Pendulum in Lines.	Correction for Temperature.	Correction for Buoyancy.	Corrected Seconds Pendulum.	Fraction Less than at Sea Level.	Fraction Given by Inverse Square Law $2h/r$.
Pichincha .	2,434	438·70	−·05	+·04	438·69	$\frac{1}{845}$	$\frac{1}{674}$
Quito . .	1,466	438·83	—	+·05	438·88	$\frac{1}{1331}$	$\frac{1}{1115}$
Isle of Inca .	—	439·07	+·075	+·06	439·21		

In a matter-free space rising above sea-level, gravity might be expected to decrease according to the inverse square law starting from the earth's centre, so that if h is the height above sea-level and r is the earth's radius the decrease should be $2h/r$. The value of this fraction is given in the last column, and on comparing it with the observed decrease in the preceding column it will be evident that gravity decreased more slowly than by the inverse square law. Centrifugal force would act in a contrary way, though, as Bouguer showed, by a negligible amount. The excess of gravity, as observed, above its value in a free space must therefore be assigned to the attraction of the matter above the sea-level.

Bouguer obtained for the value of gravity on a plateau of height h as compared with its value at sea level

$$g_1 = g\left(1 - \frac{2h}{r} + \frac{3h}{r}\frac{d}{D}\right)$$

where d is the density of the plateau and D that of the earth.

This formula seems to have dropped out of sight till it was revived by Young in 1819.

Putting it in the form

$$\frac{g - g_1}{g} = \frac{2h}{r}\left(1 - \frac{3}{2}\frac{d}{D}\right)$$

and using the values at Quito and sea-level—

$$D = \frac{3993}{850}\, d.$$

This result, though we now know it to be far too great, at least sufficed to show that the mean density of the earth was greater than that of the Cordilleras, and consequently, as Bouguer remarked, that the interior of the Earth was neither hollow nor full of water, as some physicists had maintained.

The Plumb-line Experiment.—After the account of the pendulum experiments Bouguer proceeds to discuss the methods by which the deflection of the plumb-line through mountain attraction may be measured, and he then gives the account of his own observations on Chimborazo. He points out that several methods are available, *e.g.*, the meridian altitude of a star may be observed by a quadrant with its zenith reading fixed by a plumb-line, at a point on a mountain side either to the north or to the south of the summit, and then at a point east or west of the first station out of the range of the mountain's attraction. The difference should give the deflection of the plumb-line due to the mountain. This is the method which he used himself. Or, selecting stations on the same meridian, respectively on the north and south slopes, the excess of the difference of astronomical latitude over the geodetic latitude is twice the deflection due to the mountain. This is the method afterwards used by Maskelyne and by James and Clarke. Or the stations may be selected one on the north side of one mountain, and the other on the south side of another more northerly mountain. Or, lastly, the stations may be selected on the east and west sides of a mountain. This method has never been used, but it appears worthy of notice for its ingenuity. I therefore give it in Bouguer's own words ("Figure de la Terre," p. 372, § 60):

"Enfin ce n'est pas seulement par des observations faites au Nord ou au Sud, qu'on peut découvrir si les montagnes sont capables d'agir *en distance;* c'est aussi par des observations faites à l'Orient ou à l'Occident; avec cette seule différence qu'il ne sera plus question d'observer la latitude, ou de prendre des hauteurs méridiennes d'Etoiles et qu'il ne s'agira que de la détermination exacte de l'heure. Cette dernière méthode qui peut avoir son utilité, me paroîtroit souvent préférable aux précédentes, sans qu'elle exige toujours le concours de deux Observateurs. Supposons que le premier soit situé sur le côté Occidental d'une autre

montagne ou de la même: si chacun de ces Observateurs prend soin de regler une pendule par des hauteurs correspondantes, il est évident que toutes ces hauteurs étant alterées par l'attraction que souffre la ligne verticale, chaque pendule sera reglée comme si le Méridien n'etoit pas exactement vertical et comme s'il s'étoit approché par en bas de la montagne, et éloigné par consequent par en haut. Supposé outre cela que l'attraction soit d'une minute de degré et que les deux montagnes soient sur l'Equateur, la première Horloge marquera le midi 4 secondes de tems trop tôt et l'autre 4 secondes trop tard. Ainsi faisant abstraction de la différence des longitudes qu'on découvrira aisément en mesurant par la Trigonométrie la distance des deux Observateurs et en l'evaluant en degrés et minutes, il y auroit 8 secondes de tems de différence entre les deux pendules. Si les deux montagnes au lieu d'être placées aux environs de l'Equateur etoient par 60 degrés de latitude, chaque minute d'inclinaison que produiroit l'attraction dans la ligne verticale apparente, peut apporter 8″ de différence dans le midi, et il y en auroit donc 16 entre les deux Horloges. Enfin pour juger de la quantité de l'attraction, il n'y auroit qu'à sçavoir exactement la différence qu'il y auroit entre les deux Horloges; et il suffiroit toujours, pour la découvrir, de convenir d'un signal de feu ou de quelqu'autre signal dont on pût saisir l'instant; et remarquer de part et d'autre la minute et la seconde de son apparition."

After considering several mountains, Bouguer fixed upon Chimborazo and selected as his first station a point on the south side, nearly in the same longitude as the centre of gravity, and just about the snow-line. The station was 2400 toises above sea level and 829 toises below the summit. After a most toilsome journey, Bouguer and de la Condamine, another member of the Commission who assisted him in this work, fixed their tent at the station on December 4, 1738. Their difficulties were greatly increased by the cold, and they had continually to guard against being buried by the snow. They succeeded, however, in rating their clock, and then they proceeded to take the meridian altitudes of six stars to the north and four to the south, by means of a quadrant of $2\frac{1}{2}$ feet radius. On December 16th they moved to the second station nearly to the west of the first, about 3500 toises distant, at a level 174 toises lower, and 32 seconds more south in latitude. Here the attraction of the mountain in the northerly direction was only about $\frac{1}{13}$ or $\frac{1}{14}$ of its value at the first station. Though at a lower level their condition was worse than before, for they had changed a sheltered place for one exposed to such a wind that, as Bouguer says, "il nous remplissoit les yeux de sable et il étoit continuellement sur le point d'enlever notre tentre." Nevertheless they succeeded in observing the altitudes of the same stars as before.

After applying corrections for the geodetic difference of latitude and the difference of refractions at the two stations, the results were treated thus: Let N be the true altitude of a north star, δ the plumb-line deflection, and z an error in the reading of the quadrant. Then at the first station the observed altitude was,

$$N - \delta + z \qquad (1)$$

If on the same night a south star was observed, z might be taken as the same and the observed altitude was:

$$S + \delta + z \qquad (2)$$

At the second station the observations being made at a different time, we must assume a different error z'. After correction for latitude and refraction the observed quantities are:

$$N + z' \qquad (3)$$

$$S + z' \qquad (4)$$

Then $$(3) - (1) - \{(4) - (2)\} = 2\delta.$$

The mean of very discordant values of 2δ was 15″. Correcting δ for the residual attraction, $\frac{1}{14}$th, at the second station, we may take the value of δ as 8″.

From an estimate of the volume of matter above the stations and its distance, it was calculated that with the same density as the earth it should produce a deflection of 1′ 43″.

Bouguer seemed to think that the smallness of the result might be due to light volcanic matter in the mountain, but he recognised fully the unsatisfactory nature of the work, and does not seem to have had any confidence in the result. He expressed the belief that some suitable place might be found in France or in England for a repetition of the experiment, and hoped that on his return to Europe he should find that means had been taken to throw further light on the subject.

Though this hope was unfulfilled and his experiments had no immediate effect, interest in the subject was awakened, and Bouguer's work undoubtedly inspired Maskelyne to make the celebrated Schehallien experiment now to be described.

PLUMB-LINE EXPERIMENTS.

The Schehallien Experiment by Maskelyne and Hutton.

(*Nos.* 4, 5, 6, 7, 9 & 12, *in the Bibliography.*)

In 1772 the Rev. Nevil Maskelyne, Astronomer Royal, read a paper at the Royal Society, in which he proposed a repetition of the attempt to measure the attraction of a mountain in the manner adopted by the Peruvian Commission. He suggested that a suitable locality might be found either in the Ingleborough district or in Cumberland, and obtained the appointment of a committee by the Society "to consider of a proper hill whereon to try the experiment, and to prepare everything necessary for carrying the design into execution."

As the result of the inquiries and inspection made by this Committee, the English hills were rejected in favour of Schehallien, a mountain near Rannoch, in Perthshire, which has a short ridge nearly east and west, sloping down steeply on the north and south. The summit is, according to the Ordnance Survey since made, 3547 feet above sea-level.

In the summer of 1774 the necessary instruments and a portable observatory were despatched thither. Two stations, the north about 1450 and the south about 1180 feet below the ridge and on the same meridian, were selected and the astronomical observations were made. These consisted in the determination at each of the two stations of the meridian zenith distances of a certain number of stars passing the meridian near the zenith. A survey of the hill and of the surrounding district was begun in 1774, and completed during the two following years. This gave the geodetical or geographical difference of latitude of the two stations, and, had the mountain not existed, or had its attraction been negligible, the difference of zenith distances of the same star as observed from the two stations, or its equivalent, the angle between the plumb-lines at the two, would have been the same as this geodetical difference. But the attraction of the mountain pulled the plumb-bob in opposite directions on the two sides, and so increased the astronomical difference, and this increase was the quantity sought.

Knowing, from the survey, the disposition and distance of the disturbing attracting matter, it was possible by calculation to determine the mass of the earth in terms of that of the disturbing matter.

For the determination of the zenith distances, Maskelyne used a zenith sector, an instrument designed only for stars very near the zenith. This consisted of a 10-foot telescope turning on a horizontal axis at the objective end, and provided with a divided arc of a few degrees at the eye-piece end. A plumb-line, passing through the horizontal axis hung down against the divided arc and fixed the zenith. The eye-piece was provided with a micrometer. It is, perhaps, worthy of note that at that time there was no satisfactory method of dividing an arc to degrees, and Maskelyne graduated his arc by laying off a chord equal to $\frac{1}{8}$ radius and then by continued bisection dividing its arc into 128 equal parts. In using the instrument the nearest division was always brought exactly under the plumb-line, and fractions of a division were determined by a micrometer. The sector was provided with a vertical axis and was fixed in the meridian by the aid of observations made with a quadrant, but owing to delays in effecting this through bad weather, Maskelyne was led to devise a plan of determining the meridian which is now commonplace. This consists in observing the times of transit of two known stars and adjusting the instrument till the difference coincides with the known difference of right ascensions.

At the south station, 76 observations were first made on 34 stars, and then, turning the instrument through 180° round its vertical axis, 93 observations on 39, chiefly the same stars. At the north station 68 observations on 32 stars were made with the face of the instrument west, and 100 on 37 with the face east.

After making due allowance for "precession, aberration and deviation and semi-annual solar nutation of the earth's axis," ten differences with the face of the sector west gave a mean of 54·2″, the greatest deviation being 2″. Ten differences with the face east gave a mean of 55″, the greatest deviation again being 2″. The mean of these 54·6″ was taken as the final result.

Meanwhile, the survey gave the difference of geographical latitude as 42.94″.

Whence the effect due to attraction of the disturbing matter was 11·6″.

So far, the work is Maskelyne's, but the computation of the result from these data was carried out in a most careful and ingenious way by Hutton. His plan was as follows:

From the data of the survey, he drew a contour map of the district, and, from his description, it would appear that he was the originator of this mode of representing the surface.

He then supposed a horizontal plane to be drawn through each station.

Taking one, say the south station, had the surface of the ground been everywhere level with this plane, the plumb-line

would have coincided with the geographical normal. The actual deviation towards the north might be set down as due to the horizontal northerly component of the attraction of the matter above the plane, the defect of matter where the surface fell below being considered as negative matter above it. The attraction was then calculated on the supposition that unit volume at unit distance has unit attraction, and to simplify the calculation, the matter was divided into vertical prisms by vertical cylinders, with radii increasing successively by equal amounts of $666\frac{2}{3}$ feet from the station, and vertical radial planes at angles with the meridian, such that there was a constant difference $\frac{1}{12}$ between the successive sines. It is easily shown that the northerly horizontal component of the attraction of a ring between two cylinders is $\frac{666\cdot\dot{6}}{12}$ × sum of $h/(r^2 + h^2)^{\frac{1}{2}}$ where h is the height of a prism above the horizontal plane. An ingenious mode of computing this from the sum of h/r, by the aid of a slide rule, was devised by Cavendish, who was also partly responsible for the method of dividing up the ground into prisms. He was evidently already interested in the problem which he was himself to attack in another way twenty years later.

Hutton used, in all, twenty rings round each observatory, extending to 13,000 feet away, and the results obtained were,

$$\text{For the south station } \frac{666\cdot\dot{6}}{12} \times 69\cdot967$$

$$\text{,, north ,, } \frac{666\cdot\dot{6}}{12} \times 88\cdot644,$$

the sum of these being nearly $8811\frac{2}{3}$ the attraction of the whole earth would in the same units be 4π R/3 or $\frac{2}{3}$ circumference.

The circumference was taken as 131284080 feet, a value, to one in three thousand, the same as that used by Clarke eighty years later.

Hence on the supposition of equal mean densities:

$$\frac{\text{Sum of attractions of hill}}{\text{attraction of earth}} = \frac{8811\cdot\dot{6}}{8752270} = \frac{1}{9933}$$

But Maskelyne found the ratio of the two equal to the circular measure of 11·6″ or $\frac{1}{17781}$; or $\frac{1}{17804}$ after allowing for "Centrifugal force."

$$\text{Hence, } \frac{\text{density of earth}}{\text{density of hill}} = \frac{17804}{9933} = \frac{9}{5} \text{ say.}$$

Hutton took the specific gravity of the hill as 2·5, whence the mean density of the earth comes out 4·5 times that of water.

Many years later, at Hutton's instigation, Playfair undertook a lithological survey of the hill. He found that the strata were nearly vertical, and he observed the position and quantity of each kind of rock. He also took specimens of each and determined their specific gravities. He was thus enabled to apply corrections to Hutton's calculations, and gave, as the limits between which the result lay, 4·5588 and 4·867 (*Phil. Trans.*, 1811).

Hutton's latest contribution to the subject (*Phil. Trans.*, 1821, pp. 276–292), contains his final estimate (based on Playfair's work) of 99/20 or 4·95, a result which he had previously published in his Tracts. There is also a noteworthy suggestion that one of the large pyramids in Egypt, might be employed instead of a mountain, the stations being selected a quarter of the way up from the base, the position which he had shown (*Phil. Trans.*, 1780, p. 1) to give the largest deflection. The rest of the paper is occupied with a résumé of the Schehallien experiment, and a criticism of Cavendish's arithmetic, a criticism afterwards shown by Baily to be itself erroneous.

It should be explained that Hutton was now eighty-four years old, and broken down in health. In his earlier years he was too exact and careful to have made any such criticism. He appears to have felt that Cavendish's value was being accepted in preference to his own and Maskelyne's, without due reason, and doubtless this made him more ready to discover what he fancied to be errors.

Reviewing the Schehallien experiment it is easy now to point out weak points. In the first place a larger number of astronomical observations with a closer agreement would have been more satisfactory. Then, in the second place, the calculations hardly extended far enough from the hill. The only justification in stopping at any distance is that the matter beyond would produce practically the same effect at the two stations. On examining Hutton's tables, I find that the last rings retained produce far too great a fraction of the whole difference to justify the neglect of those beyond, and it would be necessary to go some distance further, especially as from the map it would appear that the ground rises to the north some 20,000 feet away, and still within a distance giving a differential effect. And thirdly, the determinations of the specific gravity of each kind of rock, and its exact quantity and position, should all receive as much attention as the astronomical and survey part of the work. But this is practically impossible, and constitutes a weakness inherent in the method.

Notwithstanding these weak points the experiment was a great attempt to solve the problem, and considering all the labours and the difficulties of the work, much greater then than they would

be now, we must recognise that it was most admirably carried out, and that it fully deserves the high place which it occupies in the history of scientific achievements.

The Survey Experiment at Arthur's Seat by Colonel James and Captain Clarke.

(*No.* 24 *in the Bibliography.*)

On the completion of the Trigonometrical Survey of the United Kingdom, it was found that there were very sensible differences between the astronomical and geodetic latitudes at several places, and notably at Edinburgh, where there was more than 5″. The difference could usually be ascribed to the local configuration, and in order to confirm this explanation Col. James, the Superintendent of the Survey, in 1855, decided to examine the Edinburgh case more thoroughly by determining the difference of latitude on the two sides of Arthur's Seat astronomically, and comparing it with the geodetical difference given by the Survey.

A mineralogical survey of the surface strata of Arthur's Seat and the surrounding district was made by Col. James himself, the astronomical observations were made by Sergeant-Major Steel and the subsequent computations based on the contoured maps already prepared were undertaken by Captain Clarke.

The astronomical part of the work consisted in the determination of the zenith distances of a number of stars by over 400 observations at each of three stations N, A, and S, respectively on the north side, the summit, and the south side.

The zenith sector of Airy was used, an instrument which may be regarded as the modern equivalent of Maskelyne's zenith sector, a level being used however instead of a plumb-line. In Clarke's "Geodesy," pp. 182–4, will be found a description of the instrument.

The method of least squares was applied to the observations to extract the most probable values of the differences of astronomical latitudes. The latitude of the south station given by the stars observed there was 55° 56′ 26″·69, and assuming this to be correct the results in the second column of the table below were obtained, the third column giving the results of the Survey and the last the differences:

Station.	Astronomical Latitude.	Geodetical Latitude.	Difference. A – G.
S	55° 56′ 26·69″	55° 56′ 24·25″	2·44″
A	55 56 43·69	55 56 38·44	5·25
N	55 57 9·22	55 57 2·71	6·51

Evidently, then, there is a deflection of the plumb-line to the

north, or an attraction towards the south, at all three stations. This is greater even than the northerly deflection due to Arthur's Seat at the south station, a notable result to which we shall return.

To calculate the attraction due to the observed configuration Clarke used the method of Hutton, somewhat modified for greater exactness. He drew 5 circles at 100 feet intervals round each centre, then 15 more at 500 feet intervals round N and S, and 11 more at the same intervals round A. Then 7 more round N and S, and nine more round A, each having $\frac{1}{6}$ the radius of the preceding. This took him out nearly 24,000 feet. Dividing each quadrant into 12 sectors by Hutton's plan, and computing the attraction he obtained an equation for each station. If we put x for density of rocks / density of earth, and y for the deflection due to matter beyond the last rings retained, the following are the equations, the meaning of each term being written above:

A – G	Deflection of Outside Matter.	Deflection of First Twelve or Sixteen Rings.	Deflection of Outer Rings.
2·44 =	y –	4·265 x	+ 1·565 x
5·25 =	y	+ 1·691 x	+ 0·708 x
6·51 =	y	+ 3·845 x	1·393 x

Whence $x = ·5173$; $y = 3·8820$.

If the surface strata have density 2·75, the value assigned by Col. James, then:

$$\text{Mean density of earth} = 2·75 / ·5173 = 5·316.$$

Stopping short of the outer rings, *i.e.*, omitting the last term in each equation, the value of $x = ·5076$, and

$$\text{Mean density} = 5·417,$$

a difference showing the importance of the outer rings and justifying the remark that Hutton did not go far enough.

The work on which the above result is based appears to have been carried out most thoroughly and carefully. The astronomical observations agreed so closely that the probable error due to them is only $\pm$ ·054.

The calculation of the attraction of the surrounding irregularities was carried probably quite as far as was necessary when we consider our ignorance of the underlying material and the only source of considerable uncertainty is one not to be eliminated from this method—viz., the value assigned to the density of the deflecting matter, a value which may easily be wrong by as much as 5 per cent.

The General Deflection at all three Stations.—The question of the origin of the term y, the deflection due to outside matter, has no very direct bearing on our subject, but it is

of great importance in the allied subject of the deflection of the plumb-line generally by table lands and mountain ranges, and the accompanying alteration in gravity as shown by the pendulum. A short account of Col. Clarke's calculations in his search for an explanation may therefore not be out of place.

To the north of Edinburgh there is a defect of matter due to the Forth.

The hollow of this would be approximately covered by a rectangle 18 miles long, 12 miles wide, with its shorter side at 50° to the meridian, and having Arthur's Seat two miles away opposite its middle point. A plate of this size, depth h feet and density x as compared with that of the earth, would produce deflection $\cdot00727\ x\ h$.

Taking $h = 30$—the mean depth of the Forth—and $x = 1/5$, the effect of the water is to produce a deflection of $\cdot04''$ to the north-east. Taking fifteen feet as the difference between high and low water, there may be a variation of $\cdot02''$ with the state of the tide. Removing the water and filling up the rectangle with rock of specific gravity 2·5 to a depth of 150 feet, we should get a deflection greater than that due to the water by $0\cdot5''$. Hence the hollow of the Forth and the consequent northerly defect of matter will account for very little of the deflection of nearly $4''$—or $5''$, if we accept the results of the general Survey.

Turning now to the south, it is to be observed that the surface gradually rises to a mean level at the south of the country of about 1000 feet. Clarke extended the circles from the 21st to the 28th, each having $\frac{7}{6}$ the radius of the last, and this brought him to the borders of Edinburghshire, some thirteen miles away. The attraction due to the extra matter thus included was found to be $1\cdot64''$.

Further extension could not be made owing to the want of contoured maps, but Colonel Clarke thought that if it could be made, possibly the whole deflection might be accounted for.

It may be pointed out that, on the plan adopted for drawing the rings, the attraction of any ring depends only on its average height and not on its distance, so that an addition of ten more rings, the last about 60 miles radius and all of the average height of the preceding seven, would produce a deflection of $1\cdot64 \times \frac{10}{7} = 2\cdot34''$. Adding this to the $1\cdot64''$ already found, we have a total of $3\cdot98''$, or more than Clarke's value of the deflection. But this would assume that all the country south has the high average level of the borders of the counties of Edinburgh and Peebles, and would leave out of account the highlands of Perthshire now just coming within the radius on the other side. And here we are brought face to face with the question, Where is this process to end? An addition of ten rings means a multiplication of radius by 4·67, say roughly 5. Suppose we added another ten, the last with a radius

of 300 miles, we should include on one side the greater part of England and Wales, and on the other, all the Highlands; and this ten is worthy of as much consideration as either of the preceding groups of ten. Indeed, it becomes evident that we can have no certainty whatever in our explanation of any one case of deflection, taken by itself, since distant masses by their greater area may become as important as the nearer ones which, at first sight, we might regard as alone worthy of consideration. And, indeed, the process of calculation is apparently faulty in another respect. Everything tends to show that elevated ground of great extent, such as a group of mountains or a table-land, never exerts the attraction we should expect from its observed density and it is hard to resist the supposition first made by Airy (*Phil. Trans.*, 1855, pp. 101–4), that these elevated masses are really buoyed up by matter at their base, lighter than the average; that, in fact, they float on the liquid, or more probably viscous solid, interior very much as icebergs float on the sea. If this high ground is in equilibrium, neither rising nor falling, we may perhaps regard the total quantity of matter as the same as if all were uniform and at the sea level. Then the excess of horizontal attraction on a distant point, due to the elevated matter, would be neutralised by the defect due to the lighter matter below, and the elevations might be left out of the account.

But, perhaps, with sinking areas the matter below is not sufficiently buoyant and there might be an excess of attraction, while with rising areas the matter below may be more than sufficiently buoyant, and there may be a defect of attraction. In our present want of knowledge on these points it appears useless to take distant matter into account in order to explain any special case of difference between astronomic and geodetic latitude. The direction in which there appears some hope of success is rather in the experimental investigation of the deflection of the plumb-line at many points over large areas and the determination of such a distribution of excess or defect of matter as would account for all these differences.

Then we should be able to decide as to the truth of Airy's flotation theory, or at least we might find whether it was possible.

PENDULUM EXPERIMENTS.

Carlini's Experiment on Mont Cenis.

(*Nos.* 13 *and* 18 *in the Bibliography.*)

In 1821, Carlini and Plana were engaged in determining the difference of longitude between the Hospice on Mont Cenis and the Observatory at Milan by observing signals made at an inter-

mediate point. Carlini was at the Mont Cenis end, and having very accurate time for the purpose of comparison with Milan time, he made use of his stay at the Hospice to determine the value of gravity there by a Borda's pendulum. This consisted of a platinum disc suspended by a metallic thread from a "rotella" weighing only ten grains, instead of the ordinary knife edge. The length of the pendulum was measured when in position in its case by means of two microscopes, the upper sighting the suspension edge, the lower, in succession, the upper and lower edges of the disc. The length between the microscopes was measured in terms of three agreeing standard metres. The pendulum and microscope were attached to a strong wall on the ground floor, and the clock was on a pyramid of masonry rising in the middle of the room. A telescope was directed to the simple pendulum, and an inclined mirror at the same time reflected into the telescope the image of the clock pendulum so that their coincidences could be observed.

The experiments extended over twenty-five days with an interval, and thirteen independent results of length and time of swing were obtained. The mean result, when corrected from the elevation of the Hospice (1943 metres) to sea level, on the supposition that no mountain existed, was 993·708 mm., with a maximum discordance of ·032 mm. The excess due to the mountain was thus brought down, as it were, to sea level.

Biot had found 741·6151 mm. as the length of the "decimal" seconds pendulum at Bordeaux, lat. 44° 50′ 25″. Correcting to the latitude of the Hospice 45° 14′ 10″, and expressing in "sexagesimal" seconds the length of the pendulum there at sea-level should be 993·498 mm. The excess of the observed result 0·210 mm. was taken as the effect due to the mountain.

Carlini considered that the mountain might be represented by a segment of a sphere 1 geographical mile high and with a base 11 miles in diameter. The attraction of such an elevation works out to ·5020 d, where d is the density of the mountain and 1 mile is the unit of length. The attraction of the whole earth in the same units is 14394 D, where D is the mean density of the earth, and these attractions should be in the ratio of the difference 0·210 mm. to the whole length of the seconds-pendulum 993·498 mm.

The mountain consists chiefly of schist, marble, and gypsum, and Carlini obtained the specific gravities of these from a number of specimens, finding them to be for schist 2·81, for marble 2·86, and for gypsum 2·32. He took the mean 2·66 as approximately the value of d.

$$\text{Whence} \qquad D = \frac{5{\cdot}02 \times 993498 \times 2{\cdot}66}{14394 \times 210} = 4{\cdot}39$$

Sabine (Bibliography No. 11) gives a different treatment of the observations, leading to a different result. According to Sabine

the actual length of the seconds pendulum at the Hospice is 39·0992 in. From five values taken in neighbouring latitudes at Dunkirk, Paris, Bordeaux, Figeac, and Formentara, all nearly at sea-level, the sea-level value in the latitude of Mt. Cenis found by interpolation is 39·1154 in. The difference between this and the Hospice length is ·0162 in. But were the mountain cleared away the difference due to the altitude of the Hospice should be ·0238. The effect of the mountain is therefore the excess of ·0238 over ·0162 = ·0076.

$$\text{Hence } D = \frac{5{\cdot}02 \times 39{\cdot}1154 \times 2{\cdot}66}{14394 \times {\cdot}0076} = 4{\cdot}77$$

A subsequent revision by Giulio (Bibl. No. 18) led to the value 4·95.

There are very obvious weak points in this work. The variation from the mean of ·032 in a total of ·210, in only thirteen observations is somewhat large and would render the result too uncertain, even if the value at the sea-level as obtained by interpolation could be trusted. But there is still more uncertainty in the estimate of the equivalent mountain, and in the value assumed for the specific gravity of the rocks. The experiment must, I think, be regarded as interesting rather than important, and the nearness of the value to that obtained by other methods is probably accidental, and not an evidence of the exactness of the data and the assumptions.

Airy's Experiment at the Harton Pit.

(*Nos.* 14, 15, 16, 22 *and* 23 *in the Bibliography*).

In the year 1826, the idea occurred to Mr. Airy (afterwards Sir George Airy and Astronomer Royal) that the mean density of the earth might be determined by observing the difference in the rate of a pendulum at the top and bottom of a mine. Assuming the earth to consist of homogeneous concentric spherical shells, let r be the radius of the sphere through the bottom of the mine, $r + h$ that of the outer surface, D the mean density of the inner sphere, d that of the shell penetrated by the mine. Let g be the value of gravity below, and g' that above.

$$\text{Then } g / g' = \frac{4}{3}\pi r \, D / \left\{\frac{4}{3}\pi r^3 D / (r + h)^2 + 4\pi h d\right\}$$

$$= 1 + \frac{2h}{r} - \frac{3h}{r}\frac{d}{D} \text{ nearly,}$$

$$\text{and} \qquad \frac{D}{d} = \frac{3h}{r} / \left(1 + \frac{2h}{r} - \frac{g}{g'}\right)$$

To find the error in D due to an error in g, differentiate, and it is easily shown that:

$$\frac{\delta D}{D} = \frac{\delta g}{g} \cdot \frac{D}{d} \cdot \frac{r}{3h}.$$

Putting $D = 2d$, $h = \frac{1}{4}$ m, $r = 4000$ m, any error in g' is multiplied roughly by 10^4 in D.

Hence, to get within 1 per cent. in D, we must find g/g' to one in a million. This appeared possible to Airy, and in conjunction with Whewell he made plans to undertake a series of observations on the relative values of gravity at the top and bottom of a copper mine 1200 feet deep, at Dolcoath, near Camborne, in Cornwall. Two invariable pendulums were to be compared with two clocks, respectively at the top and bottom of the mine, the clocks being compared by chronometers carried to and fro. The pendulums were then to be interchanged, and any difference in their rates with the same value of gravity would thus be eliminated. After some trouble with the chronometer method of comparing the clocks, one set of observations of rate of swing was made. In the process of interchange, one of the pendulum boxes took fire in some unknown way while being raised in the shaft. It fell to the bottom and the accident terminated the experiment.

In 1828, a repetition was attempted in the same mine, Mr. Sheepshanks joining the party. The irregularity of the knife edges gave great trouble, and when the difficulty seemed to be overcome, a fall occurred in the mine which stopped the pump and the lower station was flooded, so that again the experiment had to be abandoned.

Airy published no account of the result of these first attempts, but from Drobisch's first paper it would appear that the difference of rate of the two pendulums corresponded to a mean density of 20, clearly showing that the experiment was still in the preliminary state.

No further steps were taken for many years.

Meanwhile, the method of electric signalling had come into use and rendered the comparison of distant clocks an easy matter, so that, when in 1854 Airy once more returned to the subject, one part of the experiment was simplified, and he had for the rest all the facilities afforded by his position as Astronomer Royal.

He now selected the Harton Pit, a coal mine in the county of Durham, two miles south of South Shields. An upper station was chosen in a stable about 200 yards from the mouth of the shaft, and a lower one in the same vertical in a disused gallery some 1250 feet below. An inner chamber 16 feet square was built in the stable and a similar chamber in the gallery as clock and pendulum rooms.

The plan was essentially that of the earlier attempts. Two

invariable pendulums, belonging to the Royal Society, marked respectively 1821 and No. 8, were used. At each station was a clock, in front of which one of the pendulums was hung, and their rates were compared by means of the method of coincidences.

To compare the two clocks a "journeyman" clock was fitted up with an electric circuit in the upper station, the circuit passing down the shaft to the lower station. Every 15 seconds the circuit was completed, and a galvanometer placed at the side of each comparison clock was deflected. By preconcerted signals it was known which deflections of the galvanometers were to be timed on the two clocks, so that their indications at the same set of instants were known, and, by successive comparisons at definite intervals their rates could be compared.

The invariable pendulum stands may be described as each consisting of two iron triangular frames with a common side—one triangle horizontal, the other vertical. A bar connecting their vertices strengthened the frame-work. At the vertex of the vertical triangle was fixed the support for the agate planes on which the steel knife-edges of the pendulum rested. The base rested on three bricks, with perfectly solid bearings, one at each angle, and the comparison clock was placed within the frame-work but free from it. The two stands were arranged so that the pendulums vibrated magnetic east and west.

There were in all six observers, and arrangements were made by which a continuous series of swings should be observed, night and day, each swing occupying about four hours. Some four or five coincidences were observed at the beginning and end of each swing, and the arc of vibration was also noted at the beginning and end. Between each pair of swings the amplitude was increased to its original value, and comparisons of the upper and lower clocks were made. Temperature and pressure observations were also made for the purposes of correction. In all, 82 swings were observed, divided into four groups, an interchange of pendulums taking place when each group was completed. The observer at a given station was relieved every twelve hours, so that the clock comparisons were sometimes made by different observers and personal equation came in. By the method of rotation of observers at the two stations, the effect was practically almost eliminated, but Airy considered it better to determine the personal equation of each observer from the actual comparisons and allow for it in the results.

It was originally intended to keep the two stations always at the same temperature, but through an accident a difference was allowed to exist, the lower station being on the average almost 7° F. warmer than the upper. The assumed temperature correction, if erroneous, might therefore seriously affect the result. A series of comparisons of the pendulums at Greenwich at the conclusion of the Harton Pit

experiment made at very different temperatures, showed, however, that the correction was practically right.

When all the corrections for a pendulum have been made its rate, or the number of vibrations per second, should be proportional to the square root of g. Hence we have:

$$\frac{g \text{ above}}{g \text{ below}} = \frac{(\text{rate of a pendulum above})^2}{(\text{rate of same pendulum below})^2}.$$

Denoting the pendulums used by "8" and "1821," this may be arranged as follows:

$$= \frac{\text{rate of "8" above}}{\text{rate of upper clock}} \times \frac{\text{rate of "1821" above}}{\text{rate of upper clock}}$$
$$\times \frac{\text{rate of lower clock}}{\text{rate of "8" below}} \times \frac{\text{rate of lower clock}}{\text{rate of 1821 below}}$$
$$\times \left(\frac{\text{rate of upper clock}}{\text{rate of lower clock}}\right)^2$$

for $\frac{8 \text{ above}}{8 \text{ below}} = \frac{1821 \text{ above}}{1821 \text{ below}}$, if the corrections have been truly applied. But here all the quantities are directly observed and so the ratio of gravity above to gravity below is determined.

When all the separate observations were weighted so as to give the least probable error, it was found that gravity below exceeded gravity above by $\frac{1}{19286}$, with a probable error of 1 in 270.

The next step is to express the difference of vertical attraction at the two stations, in terms of the whole earth, and of the strata surrounding the pit.

Let us first suppose the hollows in the upper surface filled up with material of the density of the surrounding strata, and the elevations removed so that the surface is truly spherical, and let us assume that the stratification is level.

Then, drawing a shell through the lower station such that the matter without it has no attraction at that station, the form of this shell will be spherical everywhere near the station, whatever may be its distant irregularities, and it is easy to see that its attraction on the upper station will be $4 \pi h d$, where h is the depth of the mine, and d the average density of the surface strata. The material within the shell may be regarded as concentrated at the centre of the earth for both stations, and so we may apply the spherical formula, given at the beginning of this account. Now, restoring the surface to its true form a correction has to be made for the difference of attractions of the removed or replaced matter at the two stations.

The ground round the Harton Pit is a table-land with only small irregularities, extending to a line of cliffs at the coast, about 70 feet high, and 12,000 feet from the mine at the nearest point.

The ground irregularities might be considered as neutralising each other at the upper station, and at the lower their effect was computed by dividing the surface into squares by lines parallel and perpendicular to the coast, each of side length h. The average elevation or depression above or below the upper station, was determined for each square by a survey, and so the effect at the station below could be computed.

For the sea, the surface was taken as fifteen feet below high water, and its effect had to be calculated for both stations.

The result was that the attraction of the shell on the upper station, in terms of the depth of the mine as unit of length was $(4\pi - 0{\cdot}044799)\ d = 12{\cdot}521569d$ in excess of its attraction at the lower station.

The attraction of the part of the earth inside the surface drawn through the lower station was 69625D at that station, and that of the whole earth at the upper station was

$$\left(69625 - \frac{8}{3}\pi\right)\mathrm{D} + 12{\cdot}5216\ d.$$

Whence $\dfrac{\text{gravity below}}{\text{gravity above}} = 1{\cdot}00012032 - {\cdot}00017984\ d/\mathrm{D}.$

But the pendulum experiments gave

$$\frac{\text{gravity below}}{\text{gravity above}} = 1{\cdot}00005185 \pm {\cdot}00000019.$$

Equating these values we obtain

$$\frac{\mathrm{D}}{d} = 2{\cdot}6266 + 0073.$$

From a detailed examination of the thickness of the strata, and a laborious determination of the specific gravity of each, by Prof. W. H. Miller, d was found to be 2·50. Whence

$$\mathrm{D} = 6{\cdot}566 \pm {\cdot}0182,$$

the last term expressing the probable error of the pendulum results.

Taking into account the ellipticity of the earth and its rotation, Prof. Stokes found a formula which only alters the third decimal place, making,

$$\mathrm{D} = 6{\cdot}565.$$

My impression with regard to this method is that Airy had too much confidence in the accuracy of the pendulum work. The clock was, judging from the description, supported on the same brick floor as the pendulum stand, and small though the transmission of energy from one to the other might be, with so nearly the same rate there might be a mutual effect altering the

frequencies. Carlini's arrangement appears decidedly better. The plan adopted of raising the pendulum knife edges from the planes, after each four hours' swing, may have been necessary, but the accuracy of the workmanship would have to be very great to ensure that the contact was the same on lowering again. From Airy's description of a preliminary fault, I should doubt whether the accuracy was sufficient for this.

Examining successive results there is discordance, which appears to bear out the supposition that the rate changed after each lifting up. But when we remember that an error of 0·1 second per day will make an error of more than 1 per cent in the final result, whereas the personal equation with one observer amounted to $\frac{1}{2}$ second in a four hours' swing, it would seem that it is impossible to arrive at the requisite accuracy unless personal equation is quite eliminated. A curious aberration of one or other of the pendulums in one of the series of experiments made afterwards at Greenwich to confirm the temperature correction may be referred to as showing the uncertainty of the pendulum work, though there is no evidence of any such large aberrations in the pit experiments.

Of course, beyond this uncertainty, there is the further and possibly greater uncertainty in the computation of the attraction, through our ignorance of the true density and distribution of the surface strata.

Von Sterneck's Experiments.

(*Nos.* 34, 35 and 39 *in the Bibliography.*)

Major von Sterneck has made some very interesting experiments to determine the variation of gravity underground in the Adalbert Shaft of the silver mine at Pribram in Bohemia and in the Abraham shaft of the silver mine near Freiberg in Saxony. He has also made determinations in a shaft at Krušná Hora in Bohemia in a shaft going nearly horizontally into a hill-side. These experiments are important not only in their results but in the most valuable method used to obtain the ratio of gravity at two stations.

The Pribram Experiments.

The Adalbert shaft is more than 1000 metres deep. In the first experiment made in 1882 three stations were selected, respectively at the surface, about 500 metres below and about 1000 metres below. To determine the change in gravity a single invariable half-second pendulum was used, and this was compared by a modification of the ordinary method of coincidences with a half-second clock carried from station to station with the pen-

dulum. At the surface was a seconds clock rated by astronomical observations, and the coincidence clock was compared with it by a pocket chronometer carried to and fro. The results were not very satisfactory, the values obtained for gravity at the two underground stations being nearly the same, though they differed in level by 456·5 metres. When the values were compared in turn with the surface value, the results obtained for the mean density of the earth were 6·28 and 5·01, the density of the surface strata being taken as 2·75 throughout, a value obtained by determinations from specimens of the rocks.

The work of 1882 is to be regarded as preliminary to that of the following year, 1883, when observations were made at four underground stations, including the two of the year before. The surface station was in a cellar under a building about 100 metres from the mouth of the shaft, and the underground stations were all within 200 metres of the vertical through that at the surface. At each station a pillar was built as a stand for the pendulum, about a meter high and $1 \times 1\frac{1}{2}$ metres broad at the top.

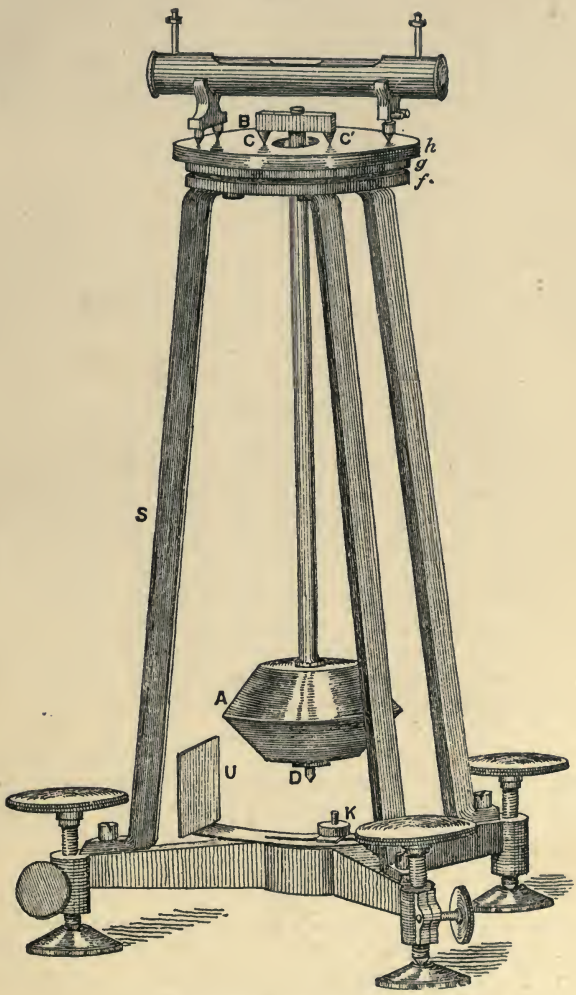
The pendulum work was on a quite new plan. Two similar invariable pendulums were used, each being a rod (fig. 1) with a heavy weight at its lower end and a knife-edge at its upper end. This was filed away in the middle so that only 2mm. of edge remained at each side. The edges rested on a glass plate with a hole in the middle through which the pendulum could be introduced and set in position. The plate was supported on a very firm tripod stand.

In a determination of gravity one pendulum was always at the surface and the other at an underground station, and the swings *were simultaneously observed in direct comparison with the same clock at the surface by means of an electric circuit.* Thus, for relative values clock errors and variations of rate are of little importance. The clock used beat seconds, and at the beginning of each second it closed an electric circuit for half a second. This circuit was carried through all the stations. The tail of the vibrating invariable pendulum moved in front of a scale and was looked at through a telescope. In the focal plane of this was a diaphragm with a horizontal slit in it. The electric current by means of a relay worked a lever carrying a second diaphragm with a slit in it, which moved up and down just in front of the first, the telescope tube being slotted so as to allow the lever to pass in. At the make and at the break, that is, at every half-second the two slits coincided for a moment and the tail of the pendulum was seen. The instant of coincidence was that at which the tail appeared in the middle of its swing when visible through the coincident slits.

When all was ready for observation the pendulums were set swinging, covered with their glass cases and left for ten or fifteen minutes. Then the observers began to note the vibrations. Two

sets of five coincidences each, separated by an interval of twelve minutes, were observed in each determination, and four such

FIG. 1.



Von Sterneck's Invariable Pendulum.

determinations were made in a day. After this the pendulums were interchanged, and four days were occupied with each underground station. Amplitude, temperature, pressure, and hygrometric state were always observed for purposes of correction.

In finding the mean density of the earth, the formula used was:

$$g = g_s \left(1 + \frac{2h}{r} - \frac{3h}{r}\frac{d}{D}\right)$$

The value of d was taken as 2·75 throughout.

The following table gives the results, the first underground station being omitted since it gave a value of gravity greater than that at the next lower. The temperature is inserted from a later paper:

Station.	Metres above Sea-level.	Metres below Surface.	Temp. R.	Gravity.	Mean Density.
Surface . . .	509	0	—	1000000	
20 Lauf . . .	− 7	516	14·78	461	5·71
26 Lauf . . .	−239	748	16·71	700	5·81
30 Lauf . . .	−463	973	19·89	903	5·80
				Mean value	5·77

The method of simultaneous observation in comparison with the same clock, devised by von Sterneck for this experiment, is an immense advance on the old method of comparison with two clocks, and it seems likely to be of great use in the relative determination of gravity at various parts of the earth's surface, now that places far apart are so easily connected by telegraph.

The paper giving an account of these experiments ends with an attempt to determine the variation of gravity within the earth from the Pribram results. Instead of assuming d to be uniform, let us take it as some function of the distance from the earth's centre. It follows that g is also a function of that distance. Assuming that the function has the form:

$$g = ar + br^2 + cr^3,$$

the four stations give four equations to determine a, b, and c. Using the method of least squares to find the most probable values and taking $g=1$ and $r=1$ at the surface, we have

$$g = 2{\cdot}6950\, r - 1{\cdot}8087\, r^2 + 0{\cdot}1182\, r^3.$$

It follows that g increases to a point ·78 radius from the earth's centre, where its value is about 1·06. Then it decreases to 0, when $r=0$. The density at any point can be found from the formula, and is given as

$$d = 15{\cdot}136 - 12{\cdot}512\, r.$$

Of course the author does not mean to put this forward as more than a hint of a possibly available method. Evidently the

assumption of a law of density as a function of r alone is untrue near the surface, and even if it were true it would be impossible to obtain with accuracy the values of the constants of the cubic when we only know definitely the values of g at $r=1$, and $r=0$, and have in addition very uncertain knowledge at three points all lying within say $\frac{1}{0000}$ part of the distance between the points $r=1$ and $r=0$. But the method is interesting, and it is conceivable that some day our knowledge may make it fruitful.

The Freiberg Experiment.

In 1871 Prof. Bruhns had found greater values of gravity at the surface than below ground in the Abraham Shaft of the Himmelfahrtsfundgrube silver mine at Freiberg. As this was an entirely anomalous result, von Sterneck determined to re-investigate the values with his two half-second invariable pendulums used simultaneously in the manner which he had devised to eliminate clock differences. As in the later Pribram experiment, the two stations at which gravity was to be compared were connected electrically with a clock sending half-second signals. These worked a relay at each station. But here a change was introduced, for in the relay circuit a spark was made at each signal. This was reflected into a telescope both by a small mirror on the pendulum knife edge perpendicular to the plane of vibration and by a fixed mirror close to the other and parallel to it when the pendulum was at rest. Coincidences were observed therefore, when both reflections appeared on the horizontal cross wire. The temperature and density of air corrections were determined for the two pendulums by actual experiments, varying the conditions first for one and then for the other, and as the results obtained at widely different times were in fair agreement the corrections might be considered known.

In November 1885 Major von Sterneck proceeded to the mine and selected a surface station and four underground stations, the surface and two of those underground being those at which Prof. Bruhns had worked. At each station a brick pillar was built 1 metre high and 60 cm. square, for the pendulum stand. An electric circuit was laid connecting the stations with a clock rated by astronomical observation.

The course of proceeding in taking observations was always the same, and like the course pursued at Pribram in 1883, each of the underground stations being compared in turn with the surface station. When the pendulums were installed, say pendulum No. 1 at the surface and No. 2 below, they were set swinging, covered with their glass cases, and left for an hour before observations were begun. Three groups of eight coincidences with intervals of twelve coincidences between the middle of successive groups were

simultaneously observed with each pendulum, amplitude, temperature, and pressure being noted at the beginning and end of a group.

The two pendulums were then dismounted, packed up, and interchanged. They were set up in their new position and left till next day, when another set of observations was made. They were then dismounted, packed up, and then set up again at the same stations without interchange, and after an interval on the same day a third set of observations was made. The pendulums were then interchanged and installed in their original positions, and the next—the third day—a concluding set of observations was made. These four sets, after all corrections were applied, gave a value of gravity above / gravity below. Twelve days sufficed to compare each of the four stations below with the surface, and then a repetition was begun; but unfortunately this was put an end to by the break-down of the electric circuit, and no further work could be done.

The following table embodies the final results:

Station.	Metres above Sea Level.	Metres below Surface.	Mean Temperature R°.	Time of Swing of the Mean Pendulum.	Value of g.	Length of Seconds Pendulum.
Surface . . .	431·93	0	variable	·5009325	1,000,000	994·0000
Stollen. . . .	334·51	97·42	9·83	303	88	88
IV. Gezeug-strecke	174·89	257·04	13·84	245	318	317
VIII. Do.	17·73	414·20	15·37	183	567	565
XI. Do.	- 102·15	534·08	18·95	127	793	788

In applying these results to find the mean density it is to be observed that the mine is situated upon a practically even plateau, the small variations in which may be disregarded. The density of the underground strata is very uniformly 2·69.

From the formula:

$$g' = g\left(1 - \frac{2h}{r} + \frac{3h}{r}\frac{d}{D}\right);$$

where g' is surface gravity, g its value at depth, h and d the density of the strata passed through, the following values are obtained by comparing each station underground in turn with the surface:

Stollen . . . . . D = 5·66
IV. Gez. . . . . D = 6·66
VIII. ,, ... D = 7·15
XI. ,, ... D = 7·60

This curious result naturally leads to the suspicion that some correction having increasing value with increasing depth is

wrongly estimated. The author examines fully the case of the temperature correction, which, if wrong, obviously satisfies this condition, and finds the values of temperature correction for the two pendulums which would give most nearly consistent values of D from the observed results. But the required corrections are decidedly smaller than the mean results of experiments in the preceding years, and further the two sets of observations repeated at stations IV. and VIII. before the break-down, are admirably in accord with the previous observations at the same stations with the old corrections, but are quite thrown out of accord by the new ones. The author therefore comes to the conclusion that a less obvious explanation must be sought, and that there is in fact an abnormal increase of gravity with depth in the locality of the experiment.

Comparing the Freiberg and Pribram results v. Sterneck points out that the increase of gravity in each case is very nearly proportional, not to depth, but to the increase in temperature. The mean value of g below $/g$ at surface is 1·0000624, and the mean underground temperature is 16·60° R. When the separate underground results are compared with the mean, they fall well into line if we assume a temperature co-efficient ·0000091 and a starting-point at 10·25° R, so that

$$g \text{ below} / g \text{ above} = 1 + 0{\cdot}0000091\ (t - 10{\cdot}25°).$$

The uppermost station, Stollen, at Freiberg, alone is excepted. Comparing observed values with those calculated from the formula, we have the following table:

—	Station.	Depth below Surface.	Gravity in Terms of Surface Value = 1	Calculated from Formula.	Differences.
Pribram. .	20 Lauf.	516	1·0000461	1·0000459	+2
	26 „	748	700	634	+66
	30 „	973	903	923	−20
Freiberg .	IV. Gez.	257	318	364	−46
	VIII. „	414	567	512	+55
	XI. „	534	793	838	−45

Von Sterneck indeed remarks that had he taken the temperature found by Bruhns in 1871 at stations IV. and XI. Gez. he could have calculated the increase of gravity at those stations with an exactness at least equal to that of the actual observations.

This very remarkable result can hardly be ascribed to any direct connection between gravity and temperature. In the first place, we may perhaps fairly compare the Harton pit result with these. The mean underground temperature there was 62.88 °F, say 13·72° R; the value of g below $/g$ above should therefore be 1·00003158, whereas Airy obtained 1·00005185, a difference four times as great

as any obtained by von Sterneck in comparing calculated and observed values. Then the various laboratory experiments conducted at various temperatures show no consistent relation. It is true that Baily's values increase with decreasing temperature, but there are other disturbing effects to be allowed for before we can draw any conclusion. On the other hand, Cornu obtained a larger value in summer than in winter, while Jolly could detect no appreciable temperature effect in the decrease of weight with height. I may say also that my own experiments gave no indication of temperature effect. As Major von Sterneck's skill and experience, as well as the consistency of his results, preclude the rejection of the relation he obtains as illusory, we must ascribe it in all probability to underground variation in density, which at the same time affects the difference in gravity above and below and the distance apart of the isothermal surfaces. In our ignorance of the conditions below the lowest level yet reached in mining, there is no difficulty in accepting the explanation.

It is, perhaps, worth while here to note the kind of irregularity in attraction which would be needed to reconcile these underground observations with other experiments.

Let g be the value of gravity at an underground station at depth h, g' its value at the surface above. Then, if G is the gravitation constant, we may put:

$$g = \frac{4}{3}\pi \, \mathrm{G} \, r \, \mathrm{D} + x$$

$$g' = \frac{4}{3}\pi \, \mathrm{G} \, r \, \mathrm{D} \left(1 - 2\frac{h}{r}\right) + 4\pi \, \mathrm{G} \, h \, d + x',$$

where x and x' are the increments due to local variations from the arrangement of the earth in homogeneous concentric spherical shells, and the other letters have their previous significance.

If we divide, to eliminate G, and remember that x and x' are small, we may easily find D in the form

$$\mathrm{D} = \frac{3d}{2 - \frac{r\,(g - g')}{hg} + \frac{r\,(x - x')}{hg}}$$

If then $x - x'$ is positive and we neglect it, we shall obtain too large a value of D. And if it increases more than in proportion to the depth—for instance, as h^2, we shall, by neglecting it, obtain an increasingly large value of D as h increases. It does not appear likely that surface irregularities, such as elevations above the upper station, can account for the observed deviations, for in all cases these have been either allowed for or neglected because of their smallness. We can then only turn to underground irre-

gularities and suppose that, in all three mines so far chosen for experiment, there exists below the mine some local increase of density producing a greater attraction at the lower than at the upper station. The peculiar temperature relation found by von Sterneck seems to support this view, for the distance apart of the isothermal surfaces with equally conducting rocks, is almost certainly regulated by conditions prevailing at some depth lower than that we can yet reach, and a variation in their distance apart implies a variation in these conditions.

We must regard as merely accidental the fact that in all three cases the observations point to an abnormal excess in the increase of gravity. Let us apply Stokes's theorem:

$$\int \mathrm{N}\, d\, \mathrm{S} = \mathrm{G} \times 4\pi\, \mathrm{M}$$

—where N is the normal attraction over a surface S and M the included matter—to a level area S′ on the surface connected by lines of force from its edge with the edge of a level surface S at an average depth h below. Then if d is the density of the included matter:

$$g'\,\mathrm{S}' - g\,\mathrm{S} = \mathrm{G}\,4\pi\,\mathrm{S}\,h\,d$$

or

$$g = g'\frac{\mathrm{S}'}{\mathrm{S}} - \mathrm{G}\,4\pi\,h\,d.$$

If the lines of force are straight lines proceeding from the earth's centre,

$$\frac{\mathrm{S}'}{\mathrm{S}} = \frac{(r+h)^2}{r^2} = 1 + \frac{2h}{r},$$

and we may take as the normal increase that given by

$$g = g'\left(1 + \frac{2h}{r}\right) - \mathrm{G}\,4\pi\,h\,d.$$

But if g increases more rapidly than this everywhere over a large area S, so that on the average

$$g = g'\left(1 + \frac{2h}{r} + \mathrm{K}\right) - \mathrm{G}\,4\pi\,h\,d;$$

$$\therefore \quad \frac{\mathrm{S}'}{\mathrm{S}} = 1 + \frac{2h}{r} + \mathrm{K},$$

so that S′ exceeds its normal value by KS, an amount proportional to the square of the linear dimensions. But since a bending outwards of the lines of force from the earth's radius, in passing up from S to S′ adds an area to the normal value of S′ proportional to the product, bending outwards × length of edge, of which the latter factor is proportional to the linear dimensions, it follows that the former factor, or bending outwards, must also be proportional

to the linear dimensions. This is contradicted by experience, for there is no cumulative divergence between the astronomical and geodetic normals. Probably, then, if a number of other mines were selected for experiment, we should find some giving abnormal defects and not excesses in the increase of gravity underground.

The true value of experiments on gravity below the earth's surface would appear to lie not in their use to determine the mean density of the earth, but rather in their indication, by anomalies, of irregularities in density in the region round the place of experiment. If, however, it were necessary to depend on this method of determining the earth's density, the application of Stokes's theorem which we have just made points out a way in which the method might be completed and rendered independent of all outside irregularities.

Starting from

$$g' S' - g S = G 4 \pi S h d,$$

divide by gS, and for g on the right hand put its value $\frac{4}{3} \pi G r D$. Then we get:

$$\frac{g'}{g} \frac{S'}{S} - 1 = \frac{3h}{r} \frac{d}{D}.$$

If, besides observing $\frac{g'}{g}$, we can also measure $\frac{S'}{S}$, we may find $\frac{d}{D}$ without any consideration of matter outside the region between S and S'. But I fear that this only points to an impracticable method of proceeding. It is just conceivable that in some colliery district a suitable ring of pits might be found sufficiently close together. Then at the surface at each pit the normal should be determined, and the variation in normal should be observed in descending the pit, say by noting the variation in level of a mercury trough by observations with a vertical telescope at the top. Thus S and S' both might be found, and the pendulum work would complete the experiment.

We shall conclude the account of Major von Sterneck's experiments with a brief notice of some pendulum work at Krušná Hora in Bohemia, in 1883. Here there is a nearly horizontal adit to an iron mine on a hill side, and gravity was determined by the Pribram method at the mouth of the adit, at stations along it and at stations at the surface overhead. The investigation hardly comes within our province as directly concerned with the mean density of the earth, but indirectly it is of interest here as affording evidence with regard to the term $3 \frac{h}{r} \frac{d}{D}$ in the Bouguer-Young formula. Faye had already questioned the necessity for this term, and if we accept Airy's view that mountains are not

external additions to a spheroidal earth composed of homogeneous layers but the exposed parts of lighter matter buoyed up and, as it were, floating on the denser substrata, we must agree that the term does not represent the true attraction.

From the values obtained at the surface at different elevations at Krušná Hora, von Sterneck calculates the value of gravity at sea level with and without the term, and he finds that its omission leads to much more consistent values. Later work at the Sagberg, in Hungary, (*Mitth. d. k. u. k. Mil. geog. Inst.*, Band v.), supports this conclusion. The underground values at Krušná Hora came nearly half-way in each case between the value at the mouth of the shaft and the value at the surface immediately above.

All this tends to confirm the conclusion that our knowledge of the distribution of the terrestrial matter is not yet sufficiently exact to enable us to obtain good values of the mean density of the earth from the observed attraction of terrestrial masses. Rather must we assume the mean density from laboratory experiments, and use the observations of terrestrial attractions for the converse problem of determining the distribution of terrestrial mass.

Mendenhall's Experiment on Fujiyama.

(*Nos.* 31 *and* 32 *in the Bibliography.*)

In the year 1880, Professor Mendenhall made determinations of the relative values of gravity at Tokio and at the summit of Fujiyama by means of a Kater's pendulum swung from the upper knife edge only, and therefore used as an invariable pendulum. The time of swing was determined by a method described in the account of an earlier experiment to determine the absolute value of gravity at Tokio (Bibl. No. 31). In this method a "trip-hammer" could be introduced at any instant under the pendulum, so that at the next passage through the lowest point the hammer was "thrown" and an electric circuit was broken. The instant of break was registered on a chronograph on which seconds were marked from a "break-circuit" chronometer. The trip-hammer was introduced at the beginning and at the end of the period of observation only, and did not interfere with the vibrations meanwhile. Thus the beginning of the first and the end of the last swings were accurately recorded, and the number of swings being known the time of a single swing was found. In general, the pendulum swung for thirty minutes, and this length of time was sufficient to give a very accurate determination. With so short a time a very small arc could be used, and as no comparison clock was needed the apparatus was rendered more portable.

At Tokio, the results were all corrected to 23·5° C. by means of an assumed coefficient of expansion, and to a barometric pressure of 30 inches by means of a curve of corrections given by Peirce (*U.S. Coast Survey Report for* 1876, Appendix No. 15). This was determined experimentally by swinging a pendulum in an enclosure at various pressures. The mean result was

$$t_1 = \cdot 999834 \text{ second.}$$

The summit of Fujiyama is much visited by pilgrims, and on it are a number of small huts used by them as temples. A priest allowed one of these to be used for the pendulum work, and the method of determining the time of swing was the same as at Tokio. The chronometer was rated by the aid of a portable transit instrument used by Prof. Chaplin, who accompanied Prof. Mendenhall. The temperature and pressure on the summit varied very little from 8·5° C. and 19·5 inches, and to these values all the observed results were corrected, the mean being

$$t_2 = 1 \cdot 000336 \text{ seconds.}$$

From the Tokio result, the sea level value at Fujiyama could be deduced by allowing for the 19′ difference in latitude and the difference in level. It was found to be

$$t_3 = \cdot 999847 \text{ second.}$$

The mountain is very conical, with a nearly constant angle of 138°. It is 2·35 miles high, and from a number of specimens the density was estimated at 2·12, this being arrived at from a knowledge of the density of the specimens when intact and when powdered up so as to exclude the air. With these as data, t_2 and t_3 give the mean density of the earth

$$\mathrm{D} = 5 \cdot 77.$$

Professor Mendenhall, recognising that the density of the mountain is a very uncertain factor, reverses the problem, and assuming Baily's value D = 5·67, he finds the density of the mountain $d = 2 \cdot 08$. Had he taken D = 5·5, d would have come out 2·02.

But the method is of course rendered uncertain by the probability that the sea-level value of gravity at Fujiyama, if the mountain were cleared away, would be different from that obtained from the Tokio value. If the Bouguer-Young correction is rejected we can certainly no longer assume that the true sea-level value is the normal spheroidal value. It would doubtless be somewhat less. The attraction of the mountain is probably therefore underestimated, and so the value $\frac{\mathrm{D}}{d}$ is over-estimated.

EXPERIMENTS WITH THE TORSION BALANCE.

The Experiment proposed by Michell.

(*See beginning of Cavendish's paper, No.* 8 *in the Bibliography.*)

The experiment carried out by Cavendish and usually named after him, was really devised, Cavendish states, by the Rev. John Michell, who completed an apparatus for the purpose, but did not live to experiment with it.

Michell's plan consisted in suspending in a narrow wooden case a horizontal rod 6 feet long, with a 2-inch sphere of lead hung at each end by a short wire. The suspending wire for the rod was 40 inches long. Outside the case were two lead spheres 8 inches in diameter. These were to be brought up opposite the suspended spheres, one on one side, the other on the other, so that their attractions on those spheres should conspire to turn the rod the same way round. Now moving each large sphere on to the other side of the case so as to pull the suspended sphere with equal force in the opposite direction, the rod should turn through twice the angle which it would describe if the spheres were taken altogether away. Hence half this angle would give the twist due to the attractions in one position alone. Knowing the torsion couple of the suspending wire for a given angle of twist and the length of the rod, the attracting force would be calculable. To find the torsion couple, Michell proposed to set the rod vibrating. From its moment of inertia and time of vibration the couple could be found.

Neglecting all corrections, the mathematics of the method may be reduced to the following.

Let the two suspended balls have mass m each, the two attracting balls mass M each. Let the rod have length $2a$ and with the suspended balls moment of inertia I; let d be the distance apart of the centres of attracting and attracted balls, and let θ be the angle through which the attraction twists the rod.

If μ is the torsion couple per radian twist, and G the gravitation constant, then

$$\mu\,\theta = \frac{2\,\mathrm{G}\,\mathrm{M}\,m\,a}{d^2}.$$

The time of vibration

$$\mathrm{N} = 2\pi\sqrt{\mathrm{I}/\mu};$$

whence, eliminating μ,

$$\frac{4\,\pi^2\,\mathrm{I}\,\theta}{\mathrm{N}^2} = \frac{2\,\mathrm{G}\,\mathrm{M}\,m\,a}{d^2}.$$

Now we may obtain another equation containing G by expressing the acceleration of gravity in terms of the dimensions and density of the earth,

$$g = G. \frac{4}{3} \pi \frac{r^3 D}{r^2} = \frac{2}{3} G D C,$$

where r is the radius, C the circumference, and D the density of the earth. Eliminating G between the last two equations and putting for g / π^2 the length of the seconds pendulum L—a useful abbreviation—we find

$$D = \frac{3}{4} \times \frac{L}{C} \times \frac{M}{\frac{m a}{d^2}} \times \frac{N^2}{1 \theta},$$

where all the terms on the right hand are known or may be measured.

Cavendish's Experiment.

(*No.* 8 *in Bibliography.*)

On Michell's death the apparatus which he had collected for his experiment came into the possession of Prof. Wollaston, who gave it to Cavendish. Cavendish determined to carry out the experiment, with certain modifications; but he found it advisable to make the greater part of the apparatus afresh, though closely following Michell's plan and dimensions.

The actual work was done in the summer of 1797 and the following spring of 1798.

He selected for the experiment, according to Baily, an out-house in his garden at Clapham Common, and within this he appears to have constructed an inner chamber to contain the apparatus, for he states that he "resolved to place the apparatus in a room which should remain constantly shut, and to observe the motion of the arm from without by means of a telescope," in order that inequalities of temperature and consequent air currents within the case should be avoided.

The torsion rod $h\ h$ (Fig. 2, reduced from the figure in Cavendish's paper) was of deal, 6 feet long, strengthened by a silver wire tying the ends to an upright $m\ g$ in the middle. The two attracted balls $x\ x$ were lead, 2 inches in diameter, and hung by short wires from the ends of the rod.

The torsion wire was $39\frac{1}{4}$ inches long, of silvered copper, and at first of such cross section as to give a time of oscillation about 15m. This was soon changed for one with a time of oscillation about 7m.

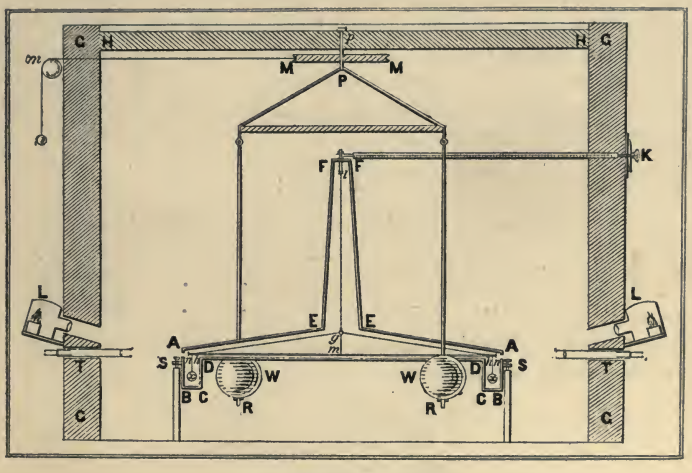
The position of the rod was determined by a fixed scale on ivory divided to $\frac{1}{20}$th inch near the end of the arm, the arm itself carrying

a vernier of five divisions. This was lighted by a lamp outside the room, and was viewed through a telescope passing through a hole in the wall.

The torsion case was supported on four levelling screws. The attracting masses, lead spheres 12 inches in diameter, W W, hung down from a cross bar, being suspended by vertical copper rods. This bar could be rotated by ropes passing outside the room round a pin fixed to the ceiling in the continuation of the torsion axis.

The masses were stopped when $\frac{1}{8}$ inch from the case by pieces

FIG. 2.



Cavendish's Apparatus.

of wood fastened to the wall of the building. When the masses were against the stops their centres were 8·85 inches from the central line of the case.

The method of experimenting varied somewhat, but we may take at random Experiment 13 as an example.

Three consecutive extremities of vibration were observed with the attracting masses in the "negative" position—*i.e.*, the position in which they tended to lower the scale reading. These were meaned in the usual way to give the centre of swing. The masses were then moved round to the positive position, and the next three extremities gave, on meaning, the new centre of swing, and the change in the position of centre was taken as the deflection.

But now a series of observations was made to determine the

time of vibration. This was done by noting the instants of passage of two consecutive divisions one on each side, if possible, of the point midway between two successive extremities of swing. By interpolation the instant of passing this midway point was found. Observing several such passages in succession, the intervals were taken as times of vibration. It is true that the point midway between two extremities in a diminishing swing is not the centre of swing, but Cavendish considered that it was reached always sensibly in the same phase of the vibration—that is, at the same time from the instant of rest—and that the interval between two successive passages would give the period, and that this would be freed practically from the effect of progression of the centre of swing. Having thus made several determinations of the period of vibration, the masses were moved back to the negative position, and the difference between the last positive and the first negative centre of swing was taken as another value of the deflection. Several more determinations of time of vibration were then made, and the experiment was brought to an end after lasting about $2\frac{1}{2}$ hours.

The results in this experiment were :

Motion of arm on moving masses from		− to +	= 6·12
,, ,, ,,		+ to −	= 5·97
Time of vibration in + position	. .		426 secs.
,, ,, − ,,	. .		427 secs.

The accuracy of the time determinations may be judged from the separate results, which were

1 in 427 1 in 424 3 in 1277 1 in 424	}	+ position
1 in 426 5 in 2139 1 in 426	}	− position

In the + position the centre of swing varied from 23·32 to 23·72, and in the negative position from 17·75 to 17·37.

In computing the results various corrections had to be introduced into the equivalents of the simple formulæ which have been given above. Taking the attraction formula :

$$\mu\theta = G\,2\,\frac{M\,m\,a}{d^2},$$

a correction had to be made, because the attracting masses were not quite opposite those attracted, as the suspending bar was a little too short. Then allowance was necessary for the attraction on the

torsion rod, and a negative correction had to be applied for the attraction on the more distant ball. The copper suspending rods were also allowed for, and a further correction was made for the change in attraction with change of scale reading—*i.e.*, for change of distance between attracting and attracted masses. This correction was proportional to the deviation from the central position, and may be regarded as an alteration of μ.

As to the case it would evidently have no effect when the rod was central, but it was necessary to examine its attraction when the rod was deflected. Cavendish found that in no case did it exceed 1/1170 of the attraction of the masses and therefore neglected it.

Turning now to the vibration formula:

$$N = 2\pi\sqrt{I/\mu};$$

this was correct when the masses were in the "midway" position—*i.e.*, in the line perpendicular to the torsion rod. But when they were in the positive or negative position, the variation in their attraction, as the balls approached or receded from them, made an appreciable alteration in the value of the restoring couple, and thus virtually altered μ. The time had therefore to be reduced by $\delta/185$ of its observed value where δ was the deflection in scale divisions due to the change of the masses from midway to near position.

But it is to be observed that if the weights were moved from one near position to the other, and the time of vibration was taken in either position, then the same correction having to be applied to μ in both formulæ, it might be omitted from both.

In all, Cavendish obtained twenty-nine results with a mean value of

$$D = 5{\cdot}448 \pm {\cdot}033.$$

By a mistake in his addition of the results, pointed out by Baily, he gave as the mean 5·48.

In Fig. 3 I have arranged the twenty-nine results in intervals of ·1 to show the closeness about the mean.

An examination of Cavendish's work in this experiment, fully bears out the general opinion that he was a magnificent experimenter. It is true that details are not given of some of the measurements, an omission adversely criticised by Baily, but I think we may trust to Cavendish's instinct for sound work, and take it for granted that he would not go to so much trouble in the calculations of small corrections if his constants were not known with corresponding accuracy. Of course we can see now that the method might have been improved in some ways, as, for instance, by introducing Baily's method of combining observations in threes, with alternate + − + positions of the masses, so as to eliminate

the effect of progressing centre of swing. And the time of vibration might perhaps have been found more exactly, but considering that it was the first attempt to measure exactly forces of such an order the success obtained was most remarkable.

Cavendish himself does not appear to have been satisfied that he had got the best work out of his apparatus, and contemplated a continuation of the experiment, but there is no record that it was ever resumed.

A very important set of observations made in the course of the work, but not bearing directly on the result, remains to be described. Cavendish had noticed that in some cases, where the large masses were left for a considerable time near the small attracted masses, the effect seemed gradually to alter, sometimes in

FIG. 3.

Distribution of Cavendish's twenty-nine results about the mean 5·448.

one direction as if indicating an increasing, sometimes in the other as if indicating a diminishing, attraction.

His first idea was that this was due to elastic after-action in the wire, but this was negatived by experiments. He then tested for magnetic effects by replacing the masses by horizontal bar magnets, but none appeared even with such a strong field as the magnets would produce. Finally he concluded that the anomalies were due to air currents through inequalities of temperature produced inside the case by the motion of the masses, which were probably of a different temperature from the case. Heating one side would give rise to an ascending current and a diminished pressure, while the descending current on the other side would be accompanied by an increased pressure. It should be noted that the march of the rod would occur while this current was growing. When the current was steady the march would cease, and there would be a certain steady deflection. When the current began to diminish there would be a return of the rod.

To find the effect of temperature inequalities, Cavendish heated the large masses with lamps, and brought them near the outside of the case containing the attracted masses. In one trial, after half an hour, the outside of the case had risen $1\frac{1}{2}$° Fahr. in temperature, while the attracted masses had been drawn through fourteen scale divisions instead of three as at first, and all the other trials gave a similar effect—a spurious attraction. The masses were cooled with ice in another experiment till 6° or 7° Fahr. below the case. When brought up against the case and left, the rod was found after an hour drawn aside $2\frac{1}{2}$ divisions less than it should have been, or there was apparently a diminishing attraction. To eliminate these effects he took the position of the rod as soon after the removal of the masses as possible, in hopes that the temperature changes would be unable to penetrate the case in time to affect the reading. He concludes these observations with the remark that "in this apparatus, the box in which the balls play is pretty deep, and the balls hang near the bottom of it, which makes the effect of the current of air more sensible than it would otherwise be, and is a defect which I intend to rectify in some future experiments."

It is interesting here to notice that Mr. Crookes found that exactly the opposite effects took place when the air was sufficiently exhausted. He says in his first paper, "On the Action of Heat on Gravitating Masses" (*Phil. Trans.*, 1874, p. 523), "A heavy metallic mass when brought near a delicately suspended light ball, attracts or repels it under the following circumstances:

"1. When the ball is in air of ordinary density.
   a. If the mass is *colder* than the ball, it *repels* the ball.
   b. If the mass is *hotter* than the ball, it *attracts* the ball.
2. When the ball is in a vacuum.
   a. If the mass is *colder* than the ball, it *attracts* the ball.
   b. If the mass is *hotter* than the ball, it *repels* the ball."

A little further on in the same paper he makes a noteworthy suggestion: "Experiment has, however, showed me that whilst the action is in one direction in dense air, and in the opposite direction in a vacuum, there is an intermediate pressure at which differences of temperature appear to exert little or no interfering action. By experimenting at this critical pressure, it would seem that such an action as was obtained by Cavendish, Reich, and Baily, should be rendered evident."

I have said that Baily's method of alternating positions and combining results in threes is superior to Cavendish's, but there is one danger in Baily's method from which Cavendish's is free. With the regularity of operation, all the various parts of the experiment occupy almost exactly the same time each time they are carried out—so at least I found in my own experiment, conducted on Baily's method—and, consequently, a given position

of the masses recurs at constant intervals. Thus inequalities of temperature will be propagated through the case in waves of period equal to that of recurrence of any given part of the experiment. Such waves may produce periodic changes in the strength of air current, and if so these will in turn produce a change in the force being measured at any particular phase of the experiment, and different changes for different phases.

In my own experiment I have not observed any positive proof of the existence of periodic disturbances arising from this source. Probably, if they were serious, the first results of a series would always differ from the mean of the series in the same way, but I have not found this to be so. There is a remarkable step-by-step descent of the centre of swing in my observations represented in Diagram 6, which may possibly be due to such a disturbance, but on comparing the results with those two days later, it does not appear to have affected the mean obtained.

Reich's First Experiment.

(*No* 17 *in the Bibliography.*)

In 1837 Professor Reich, of the Academy of Mines at Freiberg, published an account of an experiment which he had made following almost exactly the method of Cavendish.

The apparatus was set up in a large cellar under the Academy, the windows being closed up and the observer being stationed outside the room. The torsion rod was suspended from the ceiling. It was of wood, about 2 metres long, and was enclosed in a box.

The attracted balls were a composition of tin, bismuth, and lead, and weighed 484 grammes each. They were suspended 770 millimetres below the ends of the torsion rod by wires, and hung in tubes depending from the torsion-rod box. The position of the rod was read by a telescope directed through a hole in the door to a mirror at the middle of the rod which reflected a scale near the telescope. A wooden case enclosed the torsion apparatus.

The attracting masses were spheres of lead weighing 45 kilos. each, and they were suspended by long brass wires from little wheels running on rails near the ceiling *parallel to the torsion rod.* They could be moved along by ropes passing from the wheels to the observer outside the door. But as their distances from the balls were different in each of the two positions of each of the two masses, it appeared more convenient to use only one at a time, and to move it from the "null position" opposite the middle of the torsion rod to a position opposite one of the balls, termed

"plus" or "minus," according as it increased or diminished the reading.

The moment of inertia of the rod was found by the method applied by Gauss to magnets—*i.e.*, by hanging on equal weights, each about the weight of one of the balls, first from the ends of the torsion rod and then from points half-way along the two arms.

The near distance between attracting and attracted masses varied between 168 and 190 mm. The time of swing was a little over 400 seconds, and the movement of the end of the rod due to the attraction was from 0·6 to 0·8 mm.

After the measurement of distance from mass to ball was made by a kind of horizontal cathetometer, the cellar was closed up for some days before beginning observations in order that the temperature disturbances might subside. When work was to be commenced the torsion rod was set swinging round a suitable part of the scale by moving the torsion head, an adjustment which could be made from outside. Suppose the mass used to be in the "minus" position, then for the first set at least four successive extremities of swing were noted, say a, b, c, d; these were combined in the usual way equivalent to $\frac{1}{4}\ 8\ (a+3b+3c+d)$ to give the mean centre of swing. Directly the last reading, d, was taken, the mass was moved to the null position. The transfer was regarded as instantaneous, so that the same reading, d, was taken as the first of the null set. Cornu and Baille have pointed out that this method is open to serious objection, and as used by Baily led to considerable error. An account of the objection will be given in describing Baily's experiment. Taking three more readings, β, γ, δ, the new centre of swing was taken as $\frac{1}{4}(d+3\beta+3\gamma+\delta)$. Moving back to the minus position, another centre was found, and so on, the number of centres of swing thus observed in a day varying from 2 to 6. Suppose that four were found, C_1 C_2 C_3 and C_4, Reich took the value $\frac{1}{2}(C_2+C_4)-\frac{1}{2}(C_1+C_3)$ as the deflection due to the attraction of the mass.

The time of vibration was determined during the progress of the readings for each centre of swing. It was known by preliminary observations that the centre of swing lay between two given divisions, and the times between successive transits of these two divisions across the cross wire were observed. By interpolation that of the centre of swing could be found. Thus, on June 12 the centre of swing, 74·1750, was previously known to lie between 73·5 and 74·5. The following transits were noted, the second in each case not being used:

Transits of 73·5.	Transits of 74·5.	Intervals between First and Third Transits:	
		73·5	74·5
XIh. 7m. 7·2sec.	XIh. 7m. 20sec.		
,, 13m. 44sec.	,, 13m. 49sec.	810·3	809·6
,, 20m. 38sec.	,, 20m. 31·6sec.		

By interpolation the interval for 74·175 was found to be 810 secs., whence the time of oscillation was 405·000 secs.

In all, 57 determinations were made on fourteen days in June–August 1837, and the mean result for the mean density of the earth was 5·44. The means of different days varied between 5·1683 and 5·6926.

After the publication of Baily's paper, Reich (*Neue Versuche*, &c., Bibliography No. 21) adopted Baily's method of combination of centres of swing in successive threes to compute the deflection, using $C_2 - \frac{1}{2}(C_1 + C_3)$ and $\frac{1}{2}(C_2 + C_4 - C_3)$. Recomputing the results, he obtained from the same observations the value:

Mean density of the earth = 5·49

Having regard to some criticisms on Baily's experiment by Hearn (Bibl. No. 20), it is important to observe that Reich made a set of observations in which he used a cast-iron attracting sphere of 30 kilos., obtaining a result 5·43, practically agreeing with that obtained when lead was used. This clearly proved the absence of magnetic effect in the experiment with lead, for had there been any such effect with that metal, it would have been enormously greater with iron, and the results could not have been been so nearly equal.

Reich's Second Experiment.

(*No.* 21 *in the Bibliography.*)

Though not in historical order, we may here conveniently describe a continuation of the experiment made by Reich many years later with the same apparatus somewhat modified.

The work was carried out during the years 1847-1850, and the result was published in 1852.

The apparatus had been moved from the cellar to the second floor of the building, where it was under less favourable temperature conditions, but the work was carried out in the new position, the temperature changes being warded off to some extent by covering the case with tinfoil in accordance with Forbes's suggestion to Baily. At the same time electrical action was entirely prevented.

One attracting mass only was used, and this was now placed on a revolving disc or turntable, with its axis immediately under one of the attracted balls. The ball hung in a tube by a long wire from the end of the torsion rod, so that as the turntable revolved the mass could pass freely round the ball. The turntable had the same attraction on the ball in all positions, so that the correction for the suspending wires, &c., of the mass was entirely done away with.

The mode of observing was practically the same as in the first experiment, and the observations were treated by Baily's method.

The results may be classified thus:

No. of Determinations.	Suspension.	Time of Vibration.	Mean Result.
24	Copper wire ·5 mm. diameter, 2270 mm. long	About 700 secs.	5·5712
24	Copper wire 4 mm. diameter, 620 mm. long	About 500 secs.	5·6173
24	Bifilar iron 4·2 mm. apart, 2270 mm. long	About 800 secs.	5·5910

The mean of the means is $D = 5{\cdot}5832 \pm 0{\cdot}0149$.

To test for the presence of magnetic action, ten determinations were made with a diamagnetic attracted sphere of bismuth, with a result of 5·5266, and 12 with an attracted sphere of iron, with a result of 5·6887.. Reich suggests that the largeness of this result perhaps indicates a repulsion by the diamagnetic lead mass.

Reich in this, as in his earlier experiment, used the last elongation in one position of the mass as the first in the next position, a method which, as already mentioned, introduces an error, and one which tends to increase the result. But probably Reich would be able to move his masses much more rapidly than Baily, for they were only $\frac{1}{3}$ the weight. The error would thus be less, and this may perhaps explain the fact that his result is less than Baily's.

In accordance with a suggestion of Forbes, Reich made some interesting attempts to determine the attraction from the time of swing alone.

If N is the time of swing when the attracting mass is away altogether, N′ the time when it is in position at one side at a distance E (from centre to centre) and is pulling the ball a distance A towards itself, then it is easily shown that

$$N' = N\left(1 + \frac{A}{E}\right).$$

If N″ is the time of swing when the mass is placed in the vertical plane through the torsion rod and is pulling the ball outwards, it can be shown that

$$N'' = N\left(1 - \frac{A}{2E}\right),$$

$$\therefore \quad N' : N'' = 1 + \frac{A}{E} : 1 - \frac{A}{2E}.$$

If two masses be used at opposite ends of a diameter their effects are doubled, and we have:

$$N' : N = 1 + \frac{2A}{E} : 1 - \frac{A}{E},$$

whence $$A = \frac{N' - N''}{N' + 2N''} E.$$

But A is equal to $a\theta$ in the formula of p. 42, and we therefore do not require a deflection experiment.

The values of $\frac{N'}{N''}$ were very discordant, but the mean value gave

$$D = 6{\cdot}25.$$

Baily's Experiment.

(*No.* 19 *in the Bibliography.*)

The most extensive series of determinations yet published are those made by Baily at the instigation of a Committee of the Royal Astronomical Society in the years 1841–2 after several years of preparation and trial.

The apparatus, which was generally like that of Cavendish, was set up in a room in Baily's house in Tavistock Place. The window, which faced north, was doubly glazed and covered with paper, and the chimney was stopped up to prevent draughts. The torsion box was fixed to the ceiling, and was in shape like an inverted hollow T. The attracting masses were at the two ends of a plank rotating on the top of a pillar, rising from the floor under the torsion box. The torsion box, the plank and the masses, were all enclosed in a large wood case resting on the floor.

The position of the torsion rod was read by a telescope, mirror, and scale, the mirror being fixed at the centre of the rod. The telescope and scale were fixed in one corner of the room, and ropes passed to this corner from the central pillar. By these the observer at the telescope could turn the masses round.

The masses were of lead, about 12 in. in diameter, most carefully cast and turned into true spheres. They weighed about 380 lbs. each, and were 80 inches apart on the plank. Their positions relative to the torsion box were fixed by stops against which the plank moved, so that there was about 11 inches between the centres of the attracting and attracted masses.

The torsion box was lined inside with foil, then wrapped in flannel, and then surrounded by an outer wood case gilded outside. The gilding was due to a most valuable suggestion of Professor Forbes, who advised it in order that radiation from outside bodies might be in great part reflected. Temperature changes would thus penetrate the case much more gradually. At the same time all risk of electrical disturbances was abolished, for the torsion rod moved within a closed conductor.

The torsion rod was of deal, 77 inches long and braced like Cavendish's, but the balls were fixed at the ends with the axis of the rod passing through their centres. The rod generally used varied somewhat in weight at different seasons, even as much as from 2300 to 2400 grains, but this would have exceedingly little effect on the result.

The peculiarity of Baily's experiment consisted in the number of different balls and different suspensions which he employed.

Thus he used 2½-inch lead, 2-inch lead, 2-inch zinc, 2-inch glass, 2-inch ivory, 1½-inch platinum, 2½-inch hollow brass and finally a heavy torsion rod with no balls at all. For suspension he used bifilar silk different distances apart, bifilar brass, bifilar iron, and also single copper wires of different diameters. The suspending wires were usually 60 inches long. The time of vibration varied from 100 seconds to 500 seconds.

An experiment was generally conducted in the following method:

On beginning a day's work it was usually necessary to increase the oscillation of the rod by moving the attracting masses to one side and the other alternately. Suppose that when the swing was large enough, the masses were in the positive position—*i.e.*, the position in which their attraction increased the reading—then the positive end of the swing was noted, 108·90 in the example below, and the masses were immediately moved round to the negative position.

As in Reich's experiment, it was assumed that this transfer was practically instantaneous, and that 108·90 might serve as the first reading for the vibrations in the negative position. Three more were then read as recorded in the example below, and immediately on the completion of the last reading, 80·80, the masses were turned back to the positive position, and 80·80 was taken as the first of a new four for that position. This process was carried on usually for some hours with alternate + and − positions, with three readings in each, made up to four in the way described.

The four readings were then combined in the usual way, which is equivalent to $\frac{1}{4}(a + 3b + 3c + d)$, to give the mean centre of swing. Thus in Experiment 32 the results were:

Position.	Extreme Points.	Centres of Swing.	Mean.
	108·90		
Negative	82·60	95·975	95·862
	109·80	95·750	
	80·80		

The mean centre of swing in the next experiment, No. 33, with the positive position, was 113·350, and in the following, No. 34, negative, it was 95·875. Then the deviation at the time of Experiment 33 was taken as:

$$\tfrac{1}{2}\left(113{\cdot}350 - \frac{95{\cdot}862 + 95{\cdot}875}{2}\right) = 8{\cdot}741.$$

Similarly, the deviation in Experiment 34 was taken as :

$$\tfrac{1}{2}\left(\frac{(33) + (35)}{2}\right) - 34,$$

and so on.

This method was devised to eliminate the effect of progressive motion of the centre of swing which was almost always taking place, and on the assumption of the uniformity of march the method is successful.

The time of vibration was determined during the progress of each single experiment by Reich's method. Taking Experiment 32 again, it was known by preliminary observation that the centre of swing lay between 95 and 96. The times of transit of these two divisions across the cross wire were noted for three successive transits, though the middle transit was not used and is omitted below.

EXPERIMENT 32.—OBSERVED TIMES OF TRANSIT.

Division 95.			Division 96.		
10	26	34	10	26	23
10	41	41	10	41	31
2 vibrations in	15	7	in	15	8
1 vibration in	7	33·5	in	7	34

Whence, by interpolation, the time of transit of the centre of swing 95·862 was determined as 453·931. This was used as the time of vibration.

As the centre of swing was usually moving in one direction—in 32 and the following experiments in the positive direction—this method would not give quite a right result. If, for instance, the passages noted were in the same direction as the progressive motion of the centre of swing, successive passages would occur at earlier and earlier phases of the vibration, and so the calculated period would be too small. But if the passages were in the opposite direction, it would be too large. Now in successive experiments the transits were in opposite directions, and so by combining the results in threes—*i.e.*, by taking $\frac{1}{4}(t_1 + 2t_2 + t_3)$ as the time of vibration in the experiment from which t_2 was deduced—this effect was largely eliminated.

During every experiment the distances between the masses in their near position and an assumed central point were measured by means of plumb-lines and microscopes, and the mean of these was employed. The mathematical investigation justifies the use of this mean, and indeed a little consideration renders a special mathematical investigation needless. For suppose that in the +

position the distance from ball to mass is less than the mean by x, the attraction is greater than at the mean distance d by $\frac{2x}{d}$ of its value. On the negative side it is less than at the mean distance by the same fraction, and the mean value of the attraction corresponds to that at the mean distance.

In all, Baily made over 2000 determinations with various suspensions, attracted balls, and torsion rods; in each position of the masses observing the centre of swing and measuring the time of vibration and the distances.

The mathematical discussion of the vibration and deflection was contributed by Airy, who took into account variations in the restoring force due to variations in attraction of mass and of enclosing case and other fixtures. But since these equally affected the square of the time of vibration and the mean deflection due to the attraction of the mass in the two positions, they did not affect the result. It therefore only remained to compute the moment of inertia of the torsion system and the corrections to the attraction couple $M\ m\ a/d^2$ due to the planks, to the rod, etc. and using the values found, the formula on page 42 only needs modification by using a more correct value for the value of gravity in terms of the earth's dimensions, and by taking into account ellipticity and rotation.

It should be observed that the weights were corrected to vacuum. But as Routh pointed out, the weights in the air should have been used, since the masses attract the air as well as the balls. The difference, however, would be very small. If the weights used in the original weighings were correct in air, the maximum correction would be required, and even then it would not be more than 1 in about 9000 for lead, though greater for glass and ivory.

The final result was:

$$\text{Mean density of the earth} = 5{\cdot}6747 \pm {\cdot}0038.$$

I have entered somewhat into detail in describing Baily's methods of observation because there are some curious anomalies in the results which he obtained, probably to be largely explained by his methods.

The most remarkable anomaly, which evidently was a source of much trouble to Baily himself, though he never found any explanation, was the decrease in value of the mean density with increase in weight of attracted balls. Cornu and Baille (C. R. 86, p. 699) point out that this is probably due to error arising from the assumption that the last elongation for one position of the masses may be taken as the first for the succeeding position. Let us suppose that the mass is in the positive position. After taking the last reading, which is always at the positive end, the mass is moved round. But the rod has really started on its return journey, and it is only after an interval that the attraction

of the mass begins to help the torsion of the wire to pull the rod back. The result is that the rod arrives at its centre of swing with less momentum than it would have, did the attraction act throughout—a momentum corresponding to a lower starting-point—and the deduced centre of swing is too high. On the other hand, when the mass is moved from − round to + immediately after a reading at the negative end, the momentum acquired during the return corresponds to a shorter swing—*i.e.*, to a higher reading—and the deduced centre of swing is too low.

Thus, from the uniformity of Baily's method, the deduced centre of swing is always too high in the negative position and too low in the positive. Rejecting therefore the first reading in every series of 4 recorded by Baily, error from this source should be eliminated. Taking ten examples at random of experiments which appeared to be regular among themselves and which included lead, zinc, brass, ivory, and the rod alone as attracted masses, Cornu and Baille found that on rejecting the faulty reading the mean density was reduced from 5·731 to 5·615.

Reducing Baily's general mean in this ratio, it becomes 5·55. But to make the most of Baily's work it would be necessary to go through the whole of his experiments and correct them all from this point of view.

It is not quite evident how this error should vary, as it seems to do, with the weight of the balls. The time of vibration with the lighter balls would be less for a given suspension, and the time occupied in turning the masses round would be a larger fraction of the whole time, but I suspect that more than this is required to explain the variation. For we should expect that with given balls the result would depend much more evidently than it does on the time of vibration. It appears possible that the sudden withdrawal of the mass led to a reduction of pressure on that side of the case, and a consequent impulse to the ball opposing its motion.

Another very curious anomaly, a relation between the temperature and the value of the mean density, has been pointed out by Hicks (*Proc. Camb. Phil. Soc.*, vol. v. p. 156). It appears that if the results are arranged in order of the temperature where they were obtained, the mean density falls almost uniformly as the temperature rises. I have not examined this point thoroughly, but by taking out the experiments made with balls of one kind alone, as with the 2-inch lead ball, it would appear that the relation is then not quite so regular. I suspect it is partly due to the fact that a great number of the ivory and glass experiments were made in the cold weather of the winter and spring, and that the summer experiments were chiefly with heavy balls.

It is perhaps worth while pointing out that Baily did not take into account the effect of air resistance in altering the period of his torsion rod. I have examined several experiments taken at

random and, using the observed decrement of swing, I have found that the time is reduced in some cases by 1 in 1000, which would give a mean density result wrong by 1 in 500. But this error is trifling in comparison with that already noticed.

As to Baily's method of conducting the observations there is one more obvious remark. He took a long step backwards in observing from the same room and in approaching the instrument every few minutes to make his microscope measurements. It was unfortunate, with Cavendish's example before him, that he did not interpose a more efficient screen between the instrument and so large a source of disturbance as himself.

For many years this experiment was generally regarded as saying almost the last word on the subject. But the critical examination which it has received in later years has entirely destroyed any confidence in the result. It remains, however, as a most remarkable and useful example of the danger of substituting multiplication of observations for consistency.

The Experiment of Cornu and Baille (still in progress).

(*Nos.* 25–28, *and* 36 *in the Bibliography.*)

In 1870 MM. Cornu and Baille announced their intention of repeating the torsion experiment to determine the mean density of the earth. In 1873, they published a note in the *Comptes Rendus* (No. 20 in the Bibliography) describing briefly the apparatus constructed, and the results obtained up to that point. They commenced their work by a careful study of the torsion balance and of the best mode of guarding it from disturbances. They were led to the conclusion that the moment of the air resistance is accurately proportional to the velocity. They then proceeded to the construction of the gravitation apparatus, and in order to vary the conditions they made it as different as possible from that used by their predecessors Cavendish, Reich, and Baily.

The apparatus was set up in a cellar in the École Polytechnique at Paris. The torsion rod was a narrow aluminium tube 50 cm. long. The balls were of copper 109 grms. each, and the position of the rod was read by the reflection from a central mirror of a scale 5·6 metres distant. The suspending wire was of annealed silver, 4·15 metres long and such that the time of a complete vibration was 6 min. 38 sec. The attracting mass consisted of the mercury filling a hollow cast iron sphere 12cm. in diameter, the mercury being pumped from a sphere on one side to another on the other side, to avoid the vibration and air disturbance, due to the motion of heavy masses as used in previous methods. The weight of each sphere of mercury was 12 kilogrammes.

To avoid counting time, a method of electric registration was adopted, so that a graphic record was obtained giving the complete law of movement.

In July and August 1872, they obtained a mean result, 5·56, and in the following autumn and winter another mean result, 5·50. The small difference between the means they ascribed to a flexure of the rod which diminished its moment of inertia. The first set was regarded as the more trustworthy.

In the paper describing their experiment, they point out the remarkable decrease of Baily's results with increasing weights which they ascribe to an error in his calculations of the attraction of the rod. They estimate that his mean value would be reduced to 5·55 if the weights were so heavy that the rod was negligible in comparison. As we have seen, they found a key to the anomaly later.

In *Comptes Rendus*, vol. lxxxvi., 1878, they published three short notes on the continuation of their work and on the torsion balance. In the first they describe satisfactory tests of the accuracy of the law that air resistance is proportional to velocity, and they find that for a velocity of 1 cm. / sec. the resistance is about ·012 dynes / sq. cm.

In the second note they state that they have improved the gravitation apparatus by adding two more spheres of mercury and by reducing the distance of the spheres from the balls. The balance had been so perfected that for more than a year the time of vibration remained at 408 seconds within a few tenths. The mean of the results obtained was again 5·56.

It is in this paper that MM. Cornu and Baille point out the probable explanation of Baily's result of increasing density with diminishing weights, which I have already described.

The third note relates to the solution of the small oscillation equation when the restoring force has a term proportional to the square of the displacement. I pass over this, as it has no bearing on work yet published.

I have not found any later publication relating to the gravitation experiment directly, though I believe that it is now almost concluded and that the result is very near 5·5.

Boys's Experiment (still in progress).

(*No. 42 in the Bibliography.*)

A most valuable contribution to the theory of the torsion experiment was made by Professor Boys in a paper in the *Proceedings of the Royal Society*, vol. xlvi., 1889, p. 245.

In this paper for the first time, a calculation is made as to the most suitable dimensions for the apparatus. It is evident that, if the deflection can be maintained large, every diminution in the size of the apparatus down to the limits of accurate measurement is a clear gain, for the smaller the apparatus the more easily is it kept at one temperature, and the smaller are the convection currents produced when given inequalities exist, It is from this point of view that Boys takes up the subject. He says that "the sensibility of the apparatus is, if the period of oscillation is always the same, independent of the linear dimensions of the apparatus. Thus, if there are two instruments in which all the dimensions of one are n times the corresponding dimensions of the other, then the moment of inertia of the beam and its appendages will be as n^5 : 1 and, therefore, the torsion also must be as n^5 : 1. The attracting masses, both fixed and movable, will be as n^3 : 1 and their distance apart as n : 1. Therefore the attraction will be as n^6/n^2 or n^4 : 1, and this acting on an arm n times as long in the large instrument as in the small, therefore the moment will be as n^5 : 1, that is, in the same proportion as the torsion, and so the angle of deflection is unchanged.

"If, however, the length of beam only is changed, and the attracting masses are moved until they are opposite to and a fixed distance from the ends of the beam, then the moment of inertia will be altered in the ratio n^2 : 1, while the corresponding moment will only change in the ratio n : 1, and thus there is an advantage in reducing the length of the beam, until one of two things happens; either it is difficult to find sufficiently fine torsion thread that will safely carry the beam, and produce the required period, and this, I believe, has, up to the present time, prevented the use of a beam less than half a metre in length, or else, when the length becomes nearly equal to the diameter of the attracting balls, they then act with such an increasing effect on the opposite suspended balls, so as to tend to deflect the beam in the opposite direction, that the balance of effect begins to fall short of that which would be due to the reduced dimensions if the opposite ball did not interfere."

He then points out that a better effect may be obtained with a short beam (or torsion rod) if the centre of the attracting is not opposite to that of the attracted mass, but such that the line

joining the centres makes an acute angle with the prolongation of the beam. Under certain assumptions the best position is calculated.

But the difficulty of contrary moment on the more distant mass may be avoided, he shows, by having the two halves of the torsion rod at different levels and the attracting masses also at these different levels.

Further, his discovery of the method of making fine quartz fibres puts it within our power to have "a sufficiently fine torsion thread that will safely carry the beam and produce the required period."

The author then describes the beautiful instrument by which the torsion gravitation experiment, at least in the qualitative form, is rendered available even as a lecture experiment.

The torsion box is replaced by a vertical brass tube fixed on a brass base. The attracted masses are cylinders of lead 11·3 mm. long and 3 mm. in diameter, fixed by light brass arms to a light glass tube so that their axes are 6·5 mm. distant from the axis of the glass tube, and their centres are at levels differing by 50·8 mm.

A small mirror on the upper end of this tube is opposite a window in the brass tube, and the whole torsion arrangement is suspended by a quartz fibre such that the period is 80 seconds.

The attracting masses are two cylinders of lead 50·8 mm. in diameter and of the same length, fastened to the inside of a wide brass tube surrounding the narrow one and turning round its axis. A lid in two halves covers this outer tube, and the whole keeps at a very equal temperature.

In experiments with this instrument, Boys found that his anticipations were most satisfactorily verified, and he stated his intention of making another apparatus of larger dimensions, so that all the quantities could be measured to 1 in 10,000.

This apparatus has since been constructed, and the experiment has, I believe, been carried out at the Clarendon Laboratory at Oxford. The result is not yet published.

EXPERIMENTS WITH THE COMMON BALANCE.

The balance method of experiment consists in determining the increase of weight of a mass hanging from one arm of a balance, when a large attracting mass is placed below the suspended mass, so that its attraction is added to that of the rest of the earth.

The two attractions, that of the mass and that of the earth, are therefore directly compared. Knowing the dimensions and distance

of the earth, and knowing the dimensions, distance, and mass of the extra attracting body, the mass of the earth may be determined. Thus, if the mass of the earth is E, and that of the attracting body is M; if the distance of the earth's centre from the attracted body is r, and if that of the attracting body's centre (supposing it a sphere) is l; if the weight of the attracted mass is m grammes and the observed increase is w grammes, then,

$$\frac{E}{r^2} : \frac{M}{l^2} = m : w,$$

whence $$E = \frac{r^2}{l^2} \times \frac{m}{w} \times M.$$

If we regard the problem as a determination of the mass of the earth, this appears to be the most direct method.

Unfortunately it suffers from practical disadvantages which are referred to in the accounts of the various experiments.

Von Jolly's Experiment.

(*Nos.* 29 *and* 33 *in the Bibliography.*)

In 1878, Prof. v. Jolly published an account of experiments which he had made in his laboratory at Munich, to determine the variation of weight with height above the earth's surface. A balance was fixed 5·5 m. above the laboratory floor, and its case was surrounded by another case lined inside and out with silver paper. The position of the beam was determined by the reflection of a scale from a central mirror, viewed by a telescope at a distance. Under each scale-pan the case was pierced, and to the pan was attached a wire passing through the hole and supporting a second scale pan 5·29 metres lower. The wires were boxed in, and the lower pans hung in cases below. Two nickelled brass kilogrammes were first balanced against each other in the upper pans, and then one was removed to the lower pan, nearer to the earth's centre by 5·29 metres. It consequently gained in weight.

The gain observed after corrections for the air displaced, which differed in the two positions, was 1·5099 mgm. as the mean of 10 experiments, the greatest difference from the mean being ·08 mgm.

From the formula $g' = g\ (1 - 2h/r)$ the gain should have been 1·6822 mgm. The author ascribes the difference to local configuration, as the Physical Institute at Munich is at a lower level than other parts of the town. He states, in conclusion, that he intends to repeat the experiment, now building up a lead sphere of separate bars under the lower pans, and determining the increase of weight

with and without the sphere. He thus proposes to determine the mean density of the earth.

In 1881, v. Jolly published an account of a completion of this experiment, which he carried out in a tower of the University at Munich. A staircase went round the walls on the inside of the tower, leaving a square hole 1·5 metres wide down the middle, and at the top the balance was fixed.

This was constructed to carry loads of 5 kgs. on each side. Its knife edges were steel, the plates agate, and its position was read as before by mirror and scale. From each pan a wire of gilded brass hung in a zinc tube and supported another pan at its lower end, 21 metres below.

It was found that difference of temperature in these tubes produced great disturbances, especially if they were connected below. They were, therefore, wrapped in straw, and the lower pans were in separate cases.

The weights consisted of two globes containing 5 kgs. of mercury each. Four globes were, however, prepared of equal weights and equal volumes, and when two were filled with mercury all four were sealed up. To begin with, the two heavy globes were placed above, and the two light ones below, and balanced without any attracting mass below the lower ones. Then the two on one side only were interchanged and the gain in weight observed. By the use of the four globes any variation in air density the same at the same level was without effect.

The best work was on days with small temperature changes, still air, and overcast sky. When the disturbances were great work was not attempted. The experiments were always begun at 9 A.M., as the temperature was the most uniform in the morning. Ten determinations of the centre of swing were made with the weights above, the beam being lifted and released after each determination, and the mean was taken as the equilibrium position. The pair of globes on one side were then interchanged, and at 11 A.M. ten more determinations were made as before, an additional weight of 29·967 mgm. being necessary to bring the balance near to its former position, and again the mean was taken.

The sensibility of the balance being known, the difference between the positions of equilibrium in scale divisions could be converted to milligrammes, and this was added to 29·967 to give the increase in weight.

In all, fifty determinations were made between July and October 1879, and the mean result was a gain of 31·686 mgm., the separate results ranging through about ·8 mgm.

Calculation from the formula $g' = g\,(1 - 2h/r)$, gives a difference of 33·05 mgm. The smallness of the observed result may be due to high ground near, attracting at the lower position upwards

more than at the upper. This would reduce g more than g' and bring them nearer together.

A lead sphere was now built up of bars, below the lower scale pans. Its radius was 0·4975 metres, and its weight 5775·2 kg.

The weighings above and below were then carried on as before, fifty determinations being made between November 1879 and July 1880.

The mean gain in weight was 32·275 mgms. with a difference of about ·4 mgm between extremes.

The increase due to the attraction of the lead was therefore 0·589 mgm. The distance of the centre of the mercury globe from that of the lead sphere was 0·5686 metres. Using the formula on p. 61, modified to

$$\frac{4}{3}\ \pi\ r^3\ \mathrm{D} = \frac{r^2}{l^2}\ \frac{m}{w}\ \frac{4}{3}\ \pi\ b^3\ d,$$

where l is the distance ·5686 m., b the radius, and d the density of the lead sphere, Von Jolly obtained as the earth's mean density:

$$\mathrm{D} = 5{\cdot}692 \pm {\cdot}068.$$

As Richarz has pointed out (*V. J. S. der Ast. Gesellschaft Jahrgang* 24, Heft 1, p. 22), Jolly has here made a trifling arithmetical mistake, only affecting the last figure, and he has neglected the spheroidal and rotation corrections affecting the same figure.

But with so much uncertainty in the third figure it is not worth while revising the calculation in order to correct only the next figure.

Von Jolly thought that possibly the large variation might be due to hygroscopic properties of the glass. He therefore substituted metal globes empty, and filled with lead, but with no improvement.

When the difficulties of weighing accurately in an ordinary balance case are considered, we must recognise that Jolly, working with a case 20 metres high, obtained very consistent results for the gain in weight. But the value of the mean density of the earth did not depend on the total gain in weight, but on the difference in gain with and without the sphere, a quantity of only half a milligramme. The ranges of error of the two gains were respectively ·8 mgm. and ·4 mgm, so that some determinations with the sphere present were very near some when it was absent, and there was even overlapping in two cases. The quantity to be measured was in fact too small for the method employed. The temperature differences between the spheres and their surroundings, after the interchange, could hardly subside in two hours, and doubtless large disturbances were produced by the interchange.

One observation was made by v. Jolly which is worthy of

especial note. His experiments were conducted at different times of the year with large range of temperature, but without any apparent effect on the gain in weight when the mass was removed from the upper to the lower station.

König and Richarz's Experiment (still in progress by Drs. Richarz and Krigar-Menzel).

(*No.* 37 *in the Bibliography.*)

In 1884 Drs. König and Richarz independently conceived a method of using the balance, which, while adopting Jolly's principle, should increase the quantity to be measured, and decrease the disturbances.

The method consisted in first measuring the variation of gravity with height after the manner of Jolly, but with a greatly reduced height, and then in inserting a large block built up of lead between the upper and lower levels with vertical borings for the passages of the wires. Suppose now that two weights balance at the upper level. One, say the right, is moved below. The lead block pulls it up instead of down, and a small weight, w, must be added to restore equilibrium. If, on the other hand, the left weight is moved down, w must be added to the left pan. If we begin then with the right weight down and the left weight up, and then change to right up and left down, we must take w from the right and add w to the left, a total change of $2w$. But w when corrected for variation of weight before the lead block is in position, is twice the attraction of the block. The difference observed, then, is four times the attraction.

Drs. Konig and Richarz, considering from preliminary calculations that the method was very promising, arranged to carry it out together, using a nearly cubical lead block about 2 metres high.

An earth-covered casemate in the citadel of Spandau was placed at their disposal by the German Minister of War, and the balance was installed there. Dr. König, on his appointment to a professorship in the University of Berlin, retired from the work, and his place was taken by Dr. Krigar-Menzel, who is now working at the experiment in conjunction with Dr. Richarz. Until a few months ago the difficulties in determining the best mode of working, the best form of knife-edge, and the variation in weight with change of height had not been fully overcome; but from a letter which I received last March (1892) from Dr. Krigar-Menzel, I learn that the separate weighings of a kilogramme agree now within ·02 mgm. of the mean, and that the determinations of $g' - g$

for a change in height of 2·3 metres lie between $64/10^3$ and $67/10^3$ of the whole, whereas

$$\frac{g - g'}{g} = \frac{2h}{r} = \frac{71}{10^3} \text{ nearly.}$$

a difference to be ascribed to the thick walls round the locality.

The Author's Experiment.

This is a balance method. The first published account was given in 1878, in the paper No. 30 in the Bibliography; but as the experiment in its final form is fully described in Part II. (pp. 72 *et seq.*) no further reference need be made to it here.

EXPERIMENTS WITH THE PENDULUM BALANCE.

Wilsing's Experiment.

(*Nos.* 40 *and* 41 *in the Bibliography.*)

We may regard this method of experiment as most nearly allied to the torsion method, the torsion rod being replaced by a pendulum rod with balls at each end, and supported in a vertical position by a knife-edge just above the centre of gravity. The torsion couple is then replaced by the gravity couple formed by the weight and the pressure against the knife-edge. Using attracting masses, one on one side of the upper and the other on the other side of the lower ball, the pendulum is deflected. On now lowering one mass and raising the other through the length of the pendulum the deflection is reversed, and the deflection is double that due to the masses in one position alone. The value of the couple may be determined by taking the time of swing, and the torsion formulæ on p. 41 may be considered as applicable with the new interpretation of $\mu\theta$, the restoring couple.

In the apparatus devised by Wilsing, the pendulum was a brass tube 1 metre long, 4·15 cm. in diameter, and 0·16 cm. thick. In the middle were two slits through which passed the support for the pendulum knife-edge. The knife-edge was of steel, 6 cm. long and firmly attached to a frame-work fixed to the brass tube. At the ends of the pendulum were the two attracted balls, brass (Rothguss) spheres, each about 540 gms. Small movable perforated brass discs were placed on a pin projecting vertically upwards from the top of the upper ball. These were for a purpose which will be indicated below.

The experiment was conducted in a cellar of the Potsdam Astrophysical Observatory. On a foundation three feet deep a brick pillar

was built, and to one side of this a cast-iron bracket was attached carrying the support for the agate plate on which the knife-edge rested. A protecting case covered with tin surrounded the pendulum, and was provided with a window through which could be seen a mirror fixed to the pendulum just above the knife-edge. The observer was outside the cellar and viewed with a telescope through a hole in the door the reflection of a scale let into the door. The scale was on opal glass lighted from behind.

The attracting masses were cast-iron cylinders 325 kg. each, with their axes horizontal and directed to the pendulum rod. They were connected by a wire rope passing over a pulley near the ceiling, and they moved up and down in guides, coming against stops on the guides when they were exactly level with the balls. The pulley could be rotated from the outside by a suitable contrivance, and so the weights could be moved by the observer. Their axes were obliged to be inclined each at about 15° to the plane of vibration, since the bracket supporting the pendulum did not allow of their near approach when their axes were in that plane.

Before commencing work Wilsing made an interesting observation on the effect of a vessel filled with snow, placed outside the pendulum case in the plane of vibration, and found that it produced a steadily increasing deflection, which might be ascribed to the warping of the pendulum through unequal temperatures on the two sides.

The regular observations were conducted in the following manner: The masses being in one position the pendulum was lowered on to the agate support and its swinging was commenced. Four elongations gave the centre of swing. The masses were then moved to the other position, and four elongations gave the new centre. Then they were moved back, and four more elongations were observed. Then again to the second position, and four more elongations were observed. This completed a set, giving a value for the deflection. Meanwhile the times of swing were observed in each case by noting the times of transit of two scale divisions near to and one on each side, if possible, of the centre of swing. If the swings were not decreasing, the difference between two times of transit of a given division in the same direction would give the time of a double vibration. But, through the decrement of amplitude, the time observed would not be quite correct. By observing the times of transit in the opposite direction also, a time of double vibration would be obtained, affected with an opposite and nearly equal error. The mean of the four times obtained with the two sets of transits of the two divisions was taken as the true time.

The time of swing was then reduced by the removal of one of the small discs from the upper ball, and its value determined by

observing a number of transits at the beginning and a number at the end of an interval of some minutes. The logarithmic decrement was so small as to be negligible in its effect, so that it was possible to observe transits in one direction of one division only. From the times it was easy to deduce the period. A second disc was then removed, and the new and still shorter period determined in the same way. The discs were then replaced and the experiment was complete. Usually another was made later in the same day, and in all there were thirty-seven sets of observations.

In Wilsing's first paper is a very full mathematical treatment of the subject, discussing first the effect of the cylindrical form to which the so-called knife-edge really approximates, and the expression for the time of swing is found when this and buoyancy of air and attraction of masses are taken into account.

Then he shows that by removal of one of the discs and a new determination of the time of swing the value of the restoring couple may be found without a knowledge of the moment of inertia.

Neglecting all corrections, we may at once see the principle of this. The ordinary pendulum formula gives the time T_1 by

$$T_1^2 \times M g f = \pi^2 I,$$

where $M g f$ is the restoring couple per radian (supposed proportional to the angle) and I is the moment of inertia. Removing a mass m distant a from the knife-edge on the upper side, $M g f$ is increased by $m g a$, while I is diminished by $m a^2$. The time T_2 is given by

$$T_2^2 \times (M g f + m g a) = \pi^2 (I - m a^2).$$

Subtracting,

$$(T_1^2 - T_2^2) M g f = \pi^2 m a^2 - m g a T_1^2,$$

whence,

$$M g f = \frac{\pi^2 m a^2 - m g a T_2^2}{T_1^2 - T_2^2}.$$

The removal of a second disc gives another value of the same quantity, and in the actual observations the two values were in very satisfactory agreement.

After a careful and laborious investigation of the moment of the attraction, the author discusses the effect of amplitude on time of swing, theory indicating that the ordinary reduction formula must be replaced by

$$T = T_0 + x T_0^3,$$

and x is shown by experiment to have a value $a \phi - b \phi^2$, where ϕ is the amplitude, and a and b are certain constants.

Returning to the simple torsion rod formula of p. 42:

$$D = \frac{3}{4} \cdot \frac{L}{C} \cdot \frac{M m a}{d^2} \cdot \frac{N^2}{I\theta},$$

we must replace $\frac{N^2}{I}$ by $\frac{\pi^2}{Mgf}$, where Mgf has the value found above.

But this formula must now be corrected. In the first place, the value of Mgf has to be found in a form taking account of form of knife-edge, attraction of masses, and buoyancy of air, and T_1 and T_2 must be corrected by the new method to infinitely small arcs. Then, $\frac{M m a}{d^2}$ has to be replaced by the value of the attraction couple, taking into account the fact that the mass is a cylinder, and that it attracts rod as well as balls.

The value obtained from the experiment was

$$D = 5{\cdot}651 \pm 0{\cdot}017.$$

There was, however, reason to doubt the value assigned to the attraction moment on the rod without the balls. These were therefore removed, and the experiment repeated on the rod alone, the result of thirty-one sets being

$$D = 5{\cdot}731 \pm 0{\cdot}020.$$

Calling these values D_1 and D_2, we may put

$$D = D_1 + \frac{x}{m_1}\ ;\ D = D_2 + \frac{x}{m_2},$$

where x is the error in the value of the moment of attraction and $\frac{1}{m_1}$ and $\frac{1}{m_2}$ are the co-efficients of the moment in the two values for D. They are therefore known.

Hence
$$D = \frac{m_1 D_1 - m_2 D_2}{m_1 - m_2}$$
$$= 5{\cdot}594 \pm 0{\cdot}032.$$

In the second paper, the author describes a repetition of the experiment made after certain improvements in the apparatus. The pendulum support was re-arranged so that the axes of the masses were in the plane of vibration. Greater precautions were taken against temperature changes; among others, the window was blocked up, the door was lined with tin plate, and the masses were covered with tinfoil. All the constants were also carefully redetermined.

Three new series of experiments were now made, twenty-six sets with the original balls, thirty-nine with lead balls, each weighing about 745 grammes, and forty-two with the pendulum rod alone.

It was then assumed that there was an error in the value of the attraction moment, the same for the same deflection in all three cases. For the first and third series, eliminating the error as before, the result was

$$D = 5{\cdot}556 \pm 0{\cdot}026.$$

For the second and third,

$$D = 5{\cdot}584 \pm 0{\cdot}015,$$

and from both these,

$$D = 5{\cdot}588 \pm 0{\cdot}013.$$

Giving this and the previous result $5{\cdot}594 \pm 0{\cdot}032$ weights proportional to their probable errors, the final result is:

$$D = 5{\cdot}579 \pm 0{\cdot}012.$$

It might appear possible that the use of iron attracting masses was injurious through magnetic action. The author, in the first paper, shows clearly that this magnetic effect must be negligible. For in certain sets of observations one of the brass discs was replaced by an iron one of the same weight without any trace of alteration in the observations.

Laska's Proposed Experiment.

(*No.* 43 *in the Bibliography.*)

The only notice of this method which I have seen consists of a few lines in the journal referred to above. The method appears to differ from Wilsing's chiefly in two points: First, the attracting mass is the mercury filling a glass globe, and this can be withdrawn or poured in at pleasure. In the figure accompanying the description there is only one globe near the ball at the bottom of the pendulum rod, but of course it would be easy to arrange one on each side of each ball. Secondly, the position of the pendulum is read by an interference band method. The interference layer is the air between a plate on the upper end of the rod and a fixed plate close to it. The motion of the rod alters the thickness of the layer, and shifts the bands just as in Fizeau's instrument for measuring expansion.

The author describes this apparatus as having been set up, but no details are given in the note as to the quality of its performance. Indeed, I gather that it is only a trial installation, and has not been arranged to give absolute results.

SUMMARY OF RESULTS HITHERTO OBTAINED.

Approximate Date.	Experimenter.	Method.	Result.
1737–40	Bouguer	Plumb-line and Pendulum	Inconclusive
1774–6	Maskelyne and Hutton	Plumb-line	4·5 to 5
1855	James and Clarke	,,	5·316
1821	Carlini	Mountain Pendulum	4·39 to 4·95
1880	Mendenhall	,,	5·77
1854	Airy	Mine Pendulum	6·565
1883	Von Sterneck	,,	5·77
1885	Von Sterneck	,,	about 7
1797–8	Cavendish	Torsion Balance	5·448
1837	Reich	,,	5·49
1840–1	Baily	,,	5·674
1852	Reich	,,	5·583
1870–	Cornu and Baille	,,	5·56–5·50
1889	Boys	,,	in progress
1879–80	Von Jolly	Common Balance	5·692
1878–90	Poynting	,,	5·493
1884–	König, Richarz and Krigar Menzel	,,	in progress
1886–8	Wilsing	Pendulum Balance	5·579
1889	Laska	,,	in progress

PART II.

A DETERMINATION OF THE MEAN DENSITY OF THE EARTH AND THE GRAVITATION CONSTANT BY MEANS OF THE COMMON BALANCE.

(From the *Philosophical Transactions*, vol. clxxxii. A, 1891, pp. 565–656.)

I. ACCOUNT OF APPARATUS AND METHOD.

In a paper printed in the *Proceedings of the Royal Society*, No. 190, 1878 (vol. xxviii. pp. 2–35), I gave an account of some experiments undertaken in order to test the possibility of using the common balance in place of the torsion balance in the Cavendish experiment. The success obtained seemed to justify the intention expressed in that paper to continue the work, using a large bullion balance, instead of the chemical balance with which the preliminary experiments were made.

The apparatus for the experiments now to be described was first set up in the Cavendish Laboratory at Cambridge through the kindness of Professor Clerk Maxwell. After spending some months in working at the experiment, but without much success beyond the detection of some sources of error, I left Cambridge, and ultimately the apparatus was again set up at the Mason College, Birmingham. The difficulties in carrying out the work with any approach to exactness have been far greater than were anticipated, and many times work has been begun and results have been obtained, but examination has shown them to be affected by large errors which could be traced and eliminated by further improvements in the apparatus.

At the beginning of 1890, however, the apparatus was brought into fair working order, and during the course of the year I made a number of experiments with the results recorded in this paper.

The Principle of the Experiment.

The object of the experiment, in common with all of its class, may be regarded, primarily, as the determination of the attraction of one known mass M on another known mass M′ a known distance

d away from it. The law of universal gravitation states that when the masses are spheres with centres d apart this attraction is GMM'/d^2, G being a constant—the gravitation constant—the same for all masses. Astronomical observations fully justify the law as far as M'/d^2 is concerned. They do not, however, give the value of G, but only that of the product GM for various members of the solar system.

To determine G we must measure GMM'/d^2 in some case in which both M and M′ are known, whether they be a mountain and a plumb-bob, as in Maskelyne's experiment, the surface strata and a pendulum-bob, as in Airy's experiment, or two spheres of known mass and dimensions, as in all the various forms of Cavendish's experiment.

Knowing the gravitation constant G, we may at once find the mean density of the earth Δ. For if V be the volume of the earth —regarded as a sphere of radius R—the weight of any mass M′, being the attraction of the earth on it, is

$$G\,V\,\Delta\,M'/R^2.$$

But if g is the acceleration of gravity the weight is also expressible as $M'g$.

Equating these we get

$$\Delta = g\,R^2/G\,V.$$

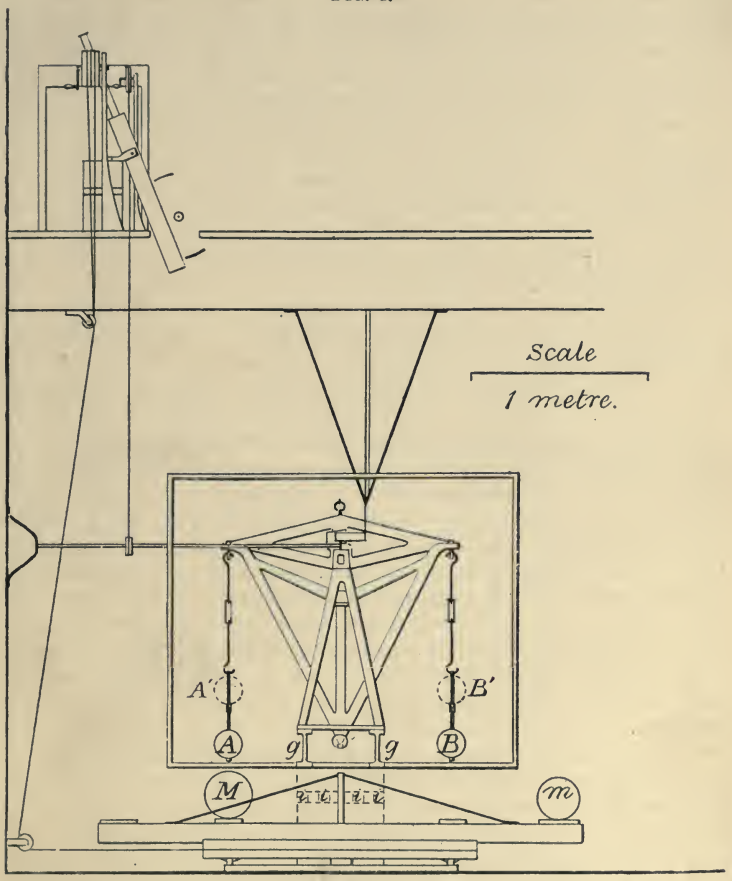
Method of using the Common Balance.

In using the common balance to find the attraction between two masses, perhaps the most direct mode of proceeding would consist in suspending a mass from one arm of a balance by a long wire, and counterpoising it in the other pan. Then bringing under it a known mass, its weight would be slightly increased by the attraction of this mass. The increase would be the quantity sought if the attracting mass had no appreciable effect before its introduction beneath the hanging mass, and if, when beneath it, the effect on the balance could be neglected. This is very nearly the principle of the method used by von Jolly, and it is that of the method used in the preliminary experiments referred to above, in which a mass of 453 grms. of lead was hung from one arm of a chemical balance (about 40 centims. beam) by a wire 1·8 metres long, and was attracted by a mass of 154 kilogrms. of lead. But the attraction to be measured was exceedingly small, rather less than 0·01 milligrm., and it therefore appeared advisable to use a much larger balance with a larger hanging mass so that the attraction might be made comparable with the weight of exactly determined riders. Other anticipations as to proportionate increase of sensibility and diminution of effect of air currents, have hardly

been justified in the way I expected, though, by the ultimate form of the apparatus, they have, I think, been more than realised.

With increase in the length of beam, a differential method

FIG. 4.



Elevation of balance room and observing room. The front of the case is removed, and the front pillar is not shown. The pointer and mirrors are at the back.

became applicable, by means of which the attraction of the mass on the beam was eliminated, and the necessity for prolonging the case to allow of a long suspending wire was removed. This will be seen from a consideration of Fig. 4. Let AB represent equal masses suspended from the two arms of the balance, and let M be

the attracting mass put first under A, the position of the beam being noted. If M is then placed under B its attraction is not only taken away from A but added to B, so that the tilting of the beam is that due to nearly double the attraction to be measured. Of course there are what we may term *cross-attractions*, in the first position, of M on B, and in the second position, of M on A, but these may be allowed for in the calculations. We cannot give any mathematical expression for the attraction of M on the beam and suspending wires, owing to their irregularity of shape. But this attraction is eliminated if a second experiment is made in which A and B are raised equal known distances to A′ and B′. For the *difference* between the two increments of weight on the right is due solely to the alteration of the positions of A and B relative to M, the attraction on the beam remaining the same in each. From the observed effect of a known alteration of distance the attraction at any distance can be found.

This is, shortly, the method adopted. The arrangement was ultimately complicated by the addition of a second mass m. Originally the mass M was alone on a turntable which revolved about a vertical axis immediately under the central knife-edge of the balance. And some experiments which I made led me to suppose that mere change of position of the mass did not affect the level of the balance. However, after a complete determination in 1888 of the mean density, when I supposed that the work was finished, an examination of the results showed some curious anomalies, which I could only ascribe to a tilting of the whole floor on the displacement of the mass. Making new tests as to the effect of removal of the mass, I found that the previous tests had been quite wrong in principle, and that there was a very appreciable effect quite visible in the telescope when the masses A and B were removed, and M was removed from one side to the other, the slope of the floor changing by an angle comparable with a third of a second. If this had been absolutely constant in amount, the differential method would have eliminated it; but, probably, it varied slightly in successive motions of the turntable, and the results showed that there was also a secular change, the amount of tilt gradually increasing. This secular change was probably due to increasing rigidity of the floor, so that it tilted over bodily, moving the supports of the balance with it, an increase partly due, perhaps, to the pressure of the building, which had only been erected ten or twelve years, but chiefly, I think, to a gas engine recently erected next door. When this was doing heavy work, the vibrations were very plainly felt, and no doubt they greatly aided the floor in "settling down." A second balancing mass m was therefore added, half as great as M, and on the opposite side of the turntable, but twice as far from the axis. The resultant pressure was now always through the axis, and I could detect no tilting of

the floor when the turntable was moved. Of course the balancing mass acted somewhat to reduce the effect of the larger attracting mass, but in a calculable ratio.

Finally, in order to eliminate or reduce the effect of any want of symmetry in the moving parts or in the masses, a second set of experiments was made with all the masses turned over and moved from left to right, and the mean of the first and second set was taken.

I now proceed to a detailed description of the various parts of the apparatus and the mode of experiment.

The Balance Room.—The balance room is in the basement of the Mason College, immediately under my room, and about 20 metres from the street. On one side were three windows looking on to a small courtyard, entirely surrounded by high buildings, but the windows have been bricked up. On the two adjacent sides are two other rooms, and on the opposite side a closely fitting door opening on a short corridor with doors at each end. There is no chimney in the room, and only an opening in the ceiling through which the balance was observed from the room above. The floor is of brick, resting on earth, and is very firmly laid.

The temperature of the room was taken by means of a thermometer with a protected bulb at the end of a long wooden rod hanging down from the room above. The thermometer was about 6 feet from the floor, near one end of the case, and it could be rapidly pulled up into the room above and read by the observer before its temperature sensibly varied. The temperature never appeared to vary so much as 0·1° C. in the course of two or three hours.

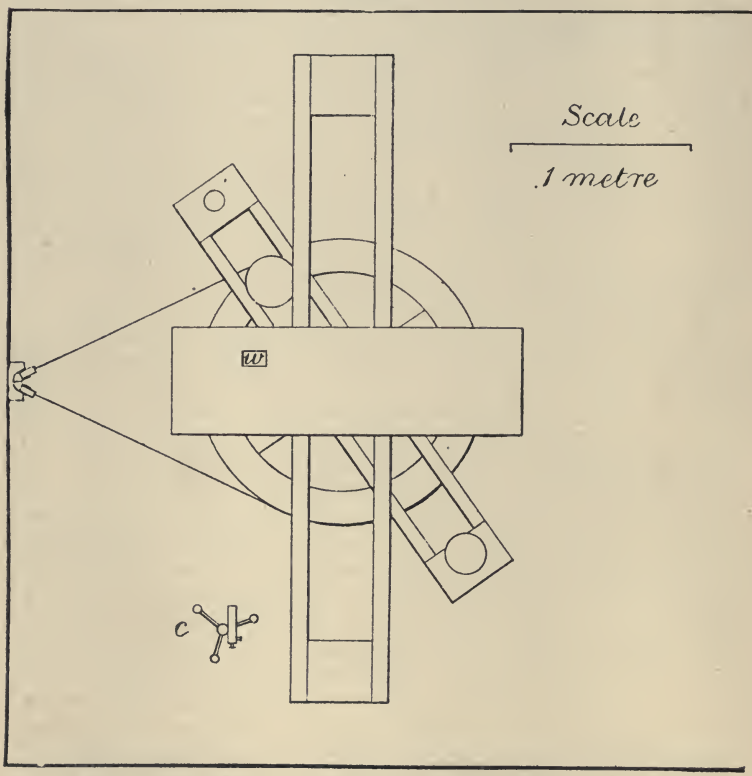
The Balance Case and its Supports.—The case (Fig. 4) is a large cabinet of $1\frac{1}{4}$ inch wood, 1·94 metre high, 1·63 metre wide, ·61 metre deep, with three large doors in front giving access to the hanging masses and riders, and a small door at the back near the mirror hereafter described. It is lined inside and out with tinfoil, and under each of the suspended masses is a double bottom with a layer of wool between, making a total thickness of about $1\frac{1}{2}$ inch or 4 centims. At the top is a small window about 10 centims. square, through which the oscillations of the beam were observed. On each side within the case are placed three horizontal partitions, like shelves, to hinder circulation of the air.

The larger attracting mass and the attracted masses are gilded, and it is possible that some advantage may arise from having the surface of the case of different metal. For if it, too, were gilded, it would readily absorb radiation from the large mass, and when the inside temperature changed, the suspended masses would readily absorb radiation from the inner surface of the case. But gold probably absorbs considerably less of tin radiation than it absorbs of gold radiation, and so temperature changes are probably lengthened out more than if the case were gilded.

It was necessary to support the case so that the attracting

masses could be moved about underneath it, and also to make it independent of the floor. Two brick pillars, 58 centims. × 36 centims. and 56 centims. high, were therefore built on thick beds of concrete under the floor, and about $3\frac{1}{2}$ metres apart. They rise up free from the brick floor. Stretching between them are two parallel iron girders (g, g), about 30 centims. apart, and with

FIG. 5.



Plan of turntable, girders, pillars, and balance case. *w*. Window in case. *c*. Usual position of cathetometer.

their under side 56 centims. above the floor. The balance case is placed across the middle of these girders (see plan, Fig. 5), with its under surface level with that of the girders. The square base plate of the balance is placed on the girders on three levelling screws. Two horizontal screws attached to the girders bear against each edge of the base plate, so that it can be adjusted and fixed in any position.

To lessen vibration one tier of bricks is removed from each pillar, and in its place are inserted eight cylindrical blocks of india-rubber (*i*, *i*, Fig. 4), originally 7·5 centims. diameter and 7·5 centims. high. These crushed down almost 1 centim. at once, but have not shown any further measurable contraction in the course of several years. Their effect in deadening vibration has been surprisingly great.

The Turntable.—On a bed of concrete, and quite free from the brickwork of the floor, is a circular rail of cast iron, 1·3 metre in diameter. On this, on conical brass wheels and pivoted at the centre, runs the turntable, about 1·5 metre in diameter. This is made of wood and covered with tinfoil. It is like a wheel with a flat circular rim, and with four flat spokes arranged as a cross. It is as nearly symmetrical as possible, and at opposite ends of a diameter are placed two shallow cups, in either of which the large attracting mass may rest. The centres of these cups are a distance apart equal to the length of the balance beam. There are slots cut through the bottom of each cup, so that the bottom of the mass can be seen for the purpose of measuring the vertical diameter.

Two beams, 2·74 metres long, run across the turntable 26 centims. apart, with the cups between them, and across the ends are two boards, each with a circular hole 12 centims. in diameter, and in either of these the smaller, or balancing mass, may rest. These beams are braced by brass rods to brass uprights at their middle points to diminish bending.

The turntable is moved by an endless gut rope passing round it, and fixed at one point of the rim. The two sides of the rope pass over pulleys on to a drum in the room above. There are stops on the circular rail, against which come brass pieces on the turntable when the masses are in position at either end of the motion. The drum can be turned easily by the observer at the telescope. Since the knife-edges and planes of the balance are of steel, all the moving parts of the apparatus are free from iron. As an illustration of the necessity of this, I may mention that for some time I used what I supposed to be a brass wire rope to move the turntable, but on looking out for the explanation of some irregularities I found that the brass was wrapped round a core of steel wire, which acquired poles at the highest and lowest points in the position in which it always rested between different sets of weighings. These poles had quite an appreciable action on the balance beam.

The Balance.—This is of the large bullion balance type, with gun-metal beam and steel knife-edges and plates. It was made specially for the experiment by Mr. Oertling, with extra rigidity of beam. Its performance has shown the great excellence of the design. The central knife-edge is supported on a steel plate by a frame-work rising 107 centims. above the base plate, and the

usual movable frame can be raised or lowered from outside the case, fixing the beam or setting it free to oscillate. The beam has often been left free to oscillate for months at a time, with the full load of 20 kilogrms. on each side, but I have no reason to suppose that the knife-edges have suffered at all.

The length of the beam was measured by taking the length of each half separately by a beam compass, and the mean of several measurements gave 123·329 centims. as the total length. The standard scale used throughout was that of a cathetometer made by the Cambridge Scientific Instrument Company. This scale has been verified at the Standards Office, and taking its coefficient of expansion as $\frac{1}{60000}$, it may be regarded for our purpose as perfectly correct at 18°, any errors being at that temperature much less than the errors of experiment. Comparing the beam compass with this scale, it was found that ·06 centim. must be subtracted, reducing the length to 123·269 centims. Now both beam and scale are of gun-metal and may, therefore, without serious error, be assumed to have the same coefficient of expansion, so that this is the length of the beam at 18°. At 0° it is 123·232 centims.

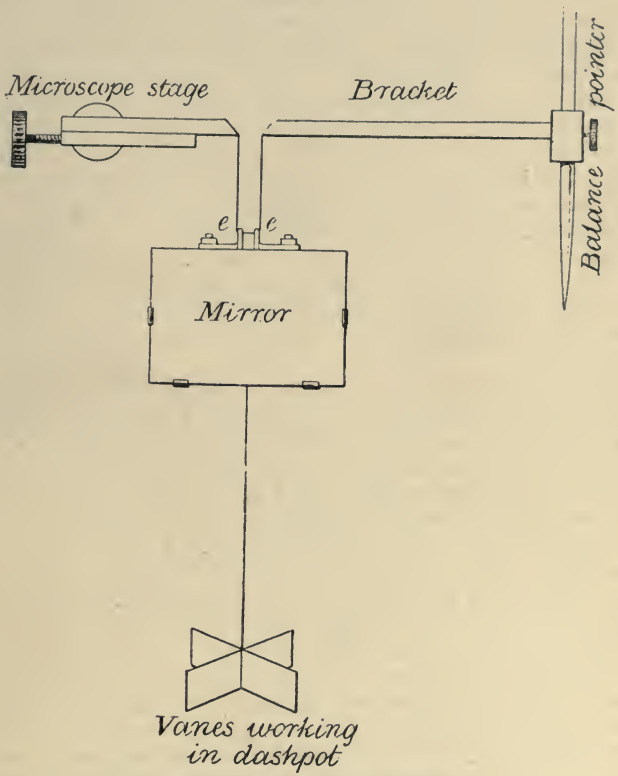
Mirrors, Telescope, and Scale.—At first a mirror was attached to the centre of the beam and the reflection of a scale in it was observed, either in the ordinary method or in the method (*Roy. Soc. Proc.*, No. 190, 1878), where a second fixed mirror is used to throw the ray of light a second or even a third time back on to the moving mirror, each return increasing the deflection of the ray. But it was then necessary to make the time of vibration very long, and even when the time was three minutes, the tilt due to the attraction—*i.e.*, the change of resting-point—did not amount to more than two or three scale divisions. Now certain irregularities observed when the apparatus was first set up at Cambridge, led to experiments on the time taken by heat to get through the case in sufficient quantity to affect the balance, and I found that a coil of copper wire placed close under the case on one side (the bottom of the case being then solid, 1 inch thickness), heated by a current yielding 100 calories per minute, began to produce an appreciable disturbance on the balance in about ten minutes, doubtless by the creation of air-currents from the heated floor of the case. It appeared advisable, therefore, to reduce the time of a complete experiment to less than this if possible, and, consequently, the time of a single swing very much below three minutes. This could only be done if at the same time the optical sensibility were very greatly increased.

The employment of what may be termed the double-suspension mirror method due, I believe, to Sir William Thomson, and used by Messrs. G. H. and Horace Darwin in their experiments on the Lunar Disturbance of Gravity (*Brit. Assoc. Rep.*, 1881), has very satisfactorily solved the problem, giving a greatly increased

deflection on the scale, even when the time of oscillation is as short as twenty seconds.

This method, which deserves to be more generally known and applied for the detection of small motions, consists in suspending a mirror by two threads, one from a fixed point, the other from the point which moves. The angle through which the mirror

FIG. 6.



Double suspension mirror (half size).

turns for a given motion of the latter point is inversely as the distance between it and the fixed point, so that by diminishing this distance the sensibility of the arrangement may be almost indefinitely increased.

To apply it to the balance, a small bracket (Fig. 6) is fixed to the ordinary pointer of the balance, about 60 centims. below the central knife-edge. This projects horizontally at right angles to the axis of the beam, and it is bevelled at the edge. Close to it is

another bevelled edge attached to a microscopic stage movement which is fixed on to the central pillar of the balance. A thread of silk (as supplied for the Kew magnetometer) is fastened to the stage, passes over the bevelled edge, through two eyes *e e*, on a light frame holding the mirror, up over the bevelled edge of the bracket, and is fastened to the bracket. The microscope stage movement allows the distance between the threads to be adjusted and also enables the azimuth of the mirror to be altered.

Of course, if the mirror were weightless, it would not affect the sensibility of the balance, and the threads might be brought very close together. But the weight of the mirror—it is silver on glass, 56 millims. × 38 millims. × 10 millims.—has a considerable effect on the sensibility, diminishing it with decrease of distance between the points of suspension. In practice it has been found convenient to work with the threads parallel, and from 3 to 4 millims. apart, the time of swing one way being adjusted to about 20 seconds. A less time hardly suffices for a correct determination and record of the scale reading. Taking four millims. as the distance, and supposing the bracket to be 600 millims. below the knife-edge of the balance, the mirror evidently turns through an angle 150 times as great as that through which the beam turns.

The drawback to this method of magnification is that the mirror has its own time of swing and is easily disturbed. The swings of the mirror and the disturbances are, however, effectually damped by having four light copper vanes attached to the end of a thin wire projecting down from the mirror and working in a dashpot with four radial partitions not quite meeting in the centre, one vane being in each compartment. I found that mineral lubricating oil is very suitable for the dashpot, as the surface keeps quite clean and there is little evaporation. The swings of the balance are also very greatly damped by this arrangement, but the effect of this will be discussed later.

The telescope and scale are in the room over the balance-room (see Fig. 4), a hole being cut through the floor, and a small glass window fixed in the top of the case. As the suspended mirror is in a vertical plane it is necessary to have an inclined mirror fixed in front of it to direct the light from the scale horizontally on to it and back again to the telescope. With the magnification used it was necessary for good definition to have an exceedingly good inclined mirror, and several were rejected before a suitable one was obtained. That finally used is a silver on glass oval mirror, 60 millims. × 40 millims., by Browning. The glass window in the case is optically worked and carefully adjusted to be normal to the path of the light.

The telescope has a 3-inch object glass of about 4-feet focal length. It is fixed on a brick pillar built on one of the brick

arches, which form the ceiling of the balance room, and it rises free from the floor of the observing room. To destroy vibration one course of bricks is replaced by blocks of india-rubber. The scale has 50 divisions to the inch (say $\frac{1}{2}$ millim.), ruled diagonally, and divided to tenths by cross lines. It is photographed on glass from a scale drawn on paper with very great care, 50 inches long (say 127 centims.), and with 500 divisions. The photograph is $\frac{1}{10}$th of this length, and only the central part of the scale, about 60 divisions in length, has been used. The diagonal ruling enables a tenth of a division to be read with certainty, and the readings recorded in the Table, pp. 129 *et seq.*, are in tenths. Though the lines appear somewhat coarse, I have not been able to find another scale equal to it in distinctness and in ease of reading.

As all the results depend on the ratio of measurements, taken almost simultaneously, of deflection due to attraction and rider respectively, in the same part of the scale, I have not thought it necessary to calibrate it.

The scale is fixed horizontally on the end of the telescope close to the object glass with a piece of ground glass over it. It was illuminated in general by an incandescent lamp placed above it, once by an Argand burner.

The distance from the scale to the mirror and back is about 5 metres. It follows that 1 division of the scale corresponds to an angular motion of the mirror through ·0001 radian. But this is at least 150 times the angle through which the beam turns for the same deflection. So that 1 scale division implies an angular motion of $\cdot 0000006\dot{}$ radian, or $\frac{2}{15}''$ in the beam. As the total length of swing in Table III. is never more than 12 divisions, the angular vibrations of the beam are at the most about 1″·6, and the linear vibrations of the masses, since the half beam is about 60 centims., are at the most about ·005 millim. This shows that it is quite unnecessary to consider any change of distance due to vibration. The greatest deviation from the mean in any of the series of weighings recorded is about one per cent. of the rider value, corresponding to about $\frac{1}{10}$ of a division, or an angle of $\frac{1}{75}''$ in the beam, and a distance of ·00004 millim., say $\frac{1}{600000}$ inch, in the motion of the masses. This seems to show that the method is accurate as well as sensitive.

Determination of the Value of the Scale Divisions by Means of Riders.—This was done by means of centigramme riders (Fig. 7), these being the least weights which appeared capable of sufficiently accurate determination. Instead of transferring the same rider from point to point, it was much easier to use two equal riders, and to take one up while the other was being let down a given distance from it. The distance selected was about 2·5 centims., since the deflection due to the transfer of one centi-

gramme so far along the beam was nearly equal to that due to the greatest attraction to be measured.

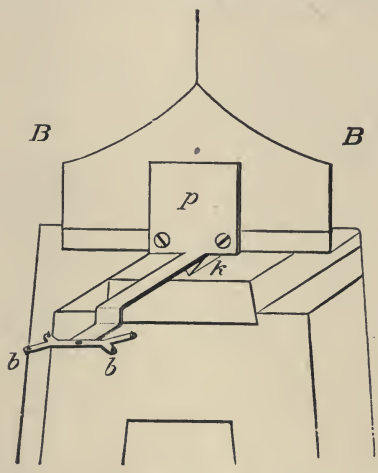
At first the riders when on the beam rested in **V** notches in a pair of parallel brass strips fixed on and parallel to the beam.

FIG. 7.

Rider, actual size, and end of lifting rod, *r*.

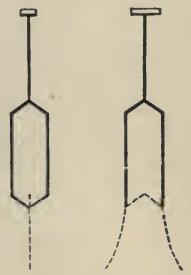
But this plan was soon abandoned, as there was no certainty about the position of the rider in the notches. The riders were then supported in little wire frames, each hung by two cocoon fibres from the edges of a plate fixed to the beam, the edges being

FIG. 8.



Subsidiary rider beam, *bb*, attached to centre of balance beam, *BB*, by plate *p* just above central knife-edge, *k* (half size).

FIG. 8*a*.



Wire frames depending like scale pans from ends of *bb*, Fig. 8, side and end views (half size).

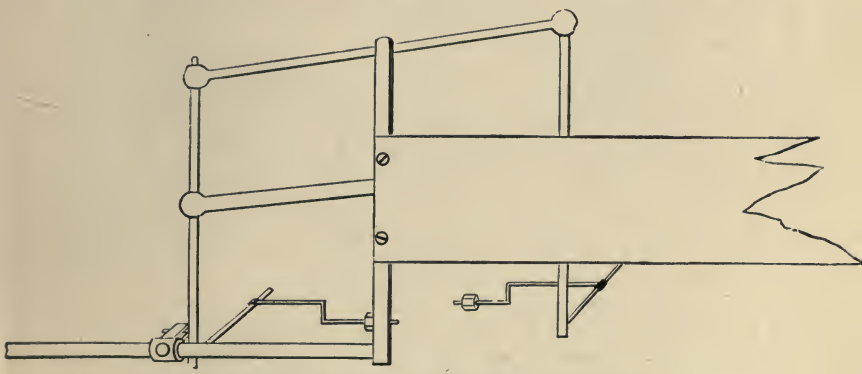
parallel to the central knife-edge. The only objection to this method was the very considerable time spent in replacing the fibres after the breakages which occurred on dusting, or any re-adjustment of the balance.

Ultimately a small subsidiary beam, about 2·5 centims. long,

was attached to the centre of the balance beam, just above the knife-edge (Fig. 8), the scale pans being represented by small wire frames in which the riders could rest (Fig. 8*a*). These frames depend from agate pieces resting on steel points at the extremities of the subsidiary beam in the way now usually adopted in delicate assay balances. This mode of supporting the riders appears to be perfectly satisfactory.

To raise or lower the riders two short horizontal lifting rods parallel to the beam move up and down within the supporting wire frames, with a nearly parallel motion, and on them are two metal pieces with their upper surfaces shaped so that the riders rest on them without swinging (Fig. 7, *r*). They are the extremities of L-shaped projections from a jointed parallelogram framework (Fig. 9), supported on an upright in front of the

FIG. 9.



Lifting rods to raise or lower riders (half size).

subsidiary beam. The framework is moved by a tongue engaging with it, and projecting from a horizontal rod, which rotates about its axis in bearings, one within the case and the other outside. The rod is turned through an angle of about 30° between stops by an endless string passing upwards and round a wheel in the observing room.

The parallelogram framework and the bearing of the rotating rod within the case, are both supported independently of the case, from the ceiling. At first they were supported respectively on the central pillar of the balance, and on the case; but when the increase of optical sensitiveness enabled me to detect small irregularities, I realised how essential it was for accurate weighing, that all parts of the apparatus moved from the outside should be supported quite independently of the balance. Even the string moving the rod transmitted great and continual vibration. The

rod and the framework with the lifting levers were, therefore, supported by iron rods coming down from the ceiling through holes in the top of the case, large pieces of cardboard stretching from these rods over the holes to hinder the passage of dust into the case. Once or twice in the course of preliminary experiments irregularities were traced to accidental contact of outside bodies with the case.

It appeared just possible that there might be electrification of the riders by friction with the lifting rods, especially when they were supported by cocoon silk. It was, therefore, advisable that the surface of the lifting rods should be of the same kind as that of the riders. As the latter are silver wire gilded, the lifting rods are also gilded. It may not be uninteresting to note here a curious phenomenon which occurred during some early preliminary experiments. The shaped pieces on the lifting rods were then of wood covered with gold leaf, put on with ordinary paste. After they had been on for some months, I obtained some very various results for the deflection due to the riders, and on examining the lifting rods, I found that a number of long needle growths projected from the wood pieces, and interfered with the supporting wire frames. At first I thought these were organic, but my colleague, Professor Hillhouse, examined them and found that they were crystalline. Doubtless, the hygroscopic paste set up electric action between the gold leaf and the brass, to which the wood pieces were attached, and the crystals were probably zinc sulphate. The wood was then replaced by brass gilded, and no further difficulty of the kind was experienced.

The length of the subsidiary beam was kindly determined for me by Mr. Glazebrook at the Cavendish Laboratory. The steel points are hardly sharp enough to determine the distance to 1 in 10,000, but the mean of the results is sufficiently exact. The following are Mr. Glazebrook's determinations; the four points being denoted by *a b c d*:—

Date.	Temperature.	Number of Readings.	*a* to *b*	Number of Readings.	*c* to *d*.
	°		Inches.		Inches.
1889 July 4 .	22·5	6	·9985	6	·9979
July 11 .	21·5	3	·9990	3	·9982
July 12 .	23	3	·9988	3	·9979

These are in terms of a gun-metal standard, of which the error is only 3 in 100,000 at 0°, and, therefore, for my purpose negligible. The beam is of brass, and we may assume with sufficient exactness that it has the same expansion as the standard. The temperature may, therefore, be left out of account. The mean

value of $\frac{1}{2}(a\,b+c\,d)$ is therefore ·9983375 inch, or taking 2·539977 centims. to the inch we obtain

Length of beam at 0°, 2·53575 centims.

There is an advantage in fixing this beam at the centre, which should be noted here. Suppose the riders are not quite equal, but have values w and $w + \delta$. Let the two ends of the subsidiary beam be distant a and $a + l$ from the central knife-edge. Then the effect of picking up the rider w from the nearer, and letting down the rider $w + \delta$ on the further end, is equivalent to putting at unit distance

$$(w + \delta)(a + l) - wa = wl + \delta(a + l) = wl\left(1 + \frac{\delta}{w}\frac{a + l}{l}\right),$$

or the error δ/w is multiplied by $(a + l)/l$, and, if the beam is not central, $(a + l)/l$ may be greater than 1, so that the error is magnified.

If, however, the small beam is central, l is equal to $-2a$, and the error is multiplied by $+\frac{1}{2}$.

If the riders are interchanged and the weighings are then repeated, the mean result is the same as if riders with the mean value were used for

$$w(a + l) - (w + \delta)a = wl - \delta a = wl\left(1 - \frac{\delta}{w}\frac{a}{l}\right),$$

and the mean of this and the above is

$$\left(w + \frac{\delta}{2}\right)l.$$

The Attracting and Attracted Masses.—These are all made of an alloy of lead and antimony, for the sake of hardness, the specific gravity in each case being about 10·4. They were made at various times and places, the large attracting mass M being made more than twelve years ago by Messrs. Storey, of Manchester. The smaller balancing mass m was made in 1889 by Messrs. Heenan, of Manchester and Birmingham. These were both cast with a "head" on, and then turned. The attracted masses A and B were made by Messrs. Whitworth, and subjected to hydraulic pressure before turning. The dimensions have been measured from time to time, and there is no evidence of any sensible change of shape.

The larger mass M and the attracted masses A and B were weighed at the Mint through the kindness of the Deputy Master and Professor Roberts-Austen. For the weight of the balancing mass m, I am indebted to Messrs. Avery, of Birmingham. The large mass M has suffered two accidents since it was weighed, once being slightly cut into by a saw during some alteration of the

case, and once being scratched by coming into contact with a piece of metal fixed to the turntable in taking it out of its place. The saw-cut was carefully filled in with lead, and the scratch removed only a fraction of a gramme, as was determined by taking a mould of the hollow. I should be glad to think that the determination of the attraction was sufficiently exact to make re-weighing necessary, but I am afraid that the alteration in weight is very far beyond the important figures, and I therefore take the original weight as sufficiently near the truth. The masses A and B have been gilded since the original weighing, but I carefully determined their increase of weight by the balance used in the gravitation experiment.

The values given below in the second column are the true masses. In the third column are the masses of M and m, less the air displaced by them, this being taken as 18·41 and 9·2 grms. respectively. It will be shown later that the true masses of A and B and the reduced masses of M and m may be used in the calculations of the result.

	True Mass in Grammes.	Mass less that of Air Displaced.
M	153407·26	153388·85
m	76497·4	76488·2
A	21582·33	
B	21566·21	

Suspension of the Attracted Masses.—Each of the attracted masses is drilled through along a diameter, the hole being ·6215 centims. in diameter, and a brass rod (Fig. 10) terminating in an eye e below, is passed through the hole. The mass is secured in position by a nut n working in a screw thread cut for a short distance in the rod. An exactly similar rod terminating in a similar eye e', and with a similar nut n', is fastened end to end to this by a union u. The nuts and the inner sides of the enlargements for the eyes are hollowed out so as to fit exactly on to the spheres.

From the ends of the balance beam hang down stout brass wires terminating in hooks. If these hooks are passed through the eyes e' the attracted masses are close to the floor of the balance case, and their centres are adjusted to be about 32 centims. from the centre of the large attracting mass when under either of them. If the masses are turned over so that the hooks pass through the eyes e, they are about 30 centims. higher or at nearly double the distance, the length ee' being about 48 centims. The rods being perfectly symmetrical about the union u, the attraction on them is the same in either position. The weight of each is about 212 grms., or about $\frac{1}{100}$ of the attracted mass, so that any small

variation in their position would produce a negligible variation in the total attraction. By the differential method, the attraction on them entirely disappears from the results.

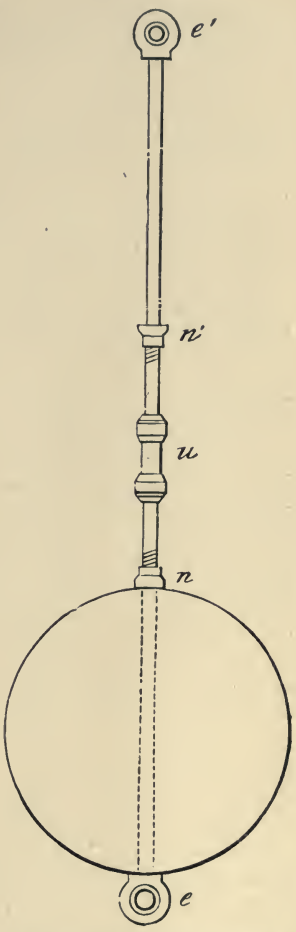
The Mode of Support of the Attracting Masses M **and** *m*.— This has already been described when describing the turntable.

The Riders. — Four centigrm. riders, A, B, C, D, of silver wire gilt were made by Mr. Oertling of the form shown in Fig. 7. These were weighed in 1886 at the Bureau International des Poids et Mesures, by M. Thiesen. The following is an extract from the certificate:—

"*Densité et Volume.*—Comme densité on a accepté celle de l'argent, et par conséquent comme volume de chacun des cavaliers, 0·0010 millilitre.

"*Détermination des poids des cavaliers.*—L'étude des poids de ces quatre cavaliers a été faite par M. le Dr. Thiesen, adjoint du Bureau International, chargé de la section des pesées. M. Thiesen au moyen de la balance Stüchrath, destinée à des poids au dessous du gramme, a d'abord déterminé les différences entre les quatre cavaliers pris deux à deux dans les six combinaisons possibles, et ensuite la différence entre l'ensemble des quatre cavaliers et le poids de 40 milligrms. de la série 0 du Bureau, série en platine iridié récemment étalonnée par M. Thiesen. Les comparaisons ont été faites du 19 au 29 Mars, 1886.

FIG. 10.



Suspender for Attracted Mass (one-fourth size).

"*Résultats.*—De l'ensemble de ces comparaisons résultent les poids:—

A = 10·1247 milligrms
B = 10·0615 „
C = 10·1196 „
D = 10·1262 „

"L'incertitude de ces déterminations ne dépasse pas 0·001 milligrm."

A and D were selected for use as being the nearest to each other in value. B and C were kept untouched in boxes till 1890. In the various experiments made between 1886 and the final weighings, A and D had necessarily been handled to some extent, especially through the frequent breaking of the silk fibre suspension used before the subsidiary beam described above, and it appeared possible that their weights might be altered. It was also necessary to determine whether an appreciable amount of dust was deposited on them in the course of several weeks as it was inconvenient to dust them frequently. The riders B and C might be assumed to have the same weight as in 1886, and could be taken as standards.

To make the weighings, a 16-inch chemical balance was arranged with a double suspension mirror on exactly the principle already described for the large balance. The apparatus was put together quickly with materials at hand, and might easily be greatly improved. It is only described here to show how accurate the method is, even with such rough apparatus, and that it is applicable to a small as well as a large balance.

A cork sliding on the pointer with a horizontal needle stuck in it, served to support one thread of the mirror; a stand with a projecting arm—one made to hold platinum wires in a Bunsen flame—served to support the other thread. A wire with a small copper vane depended from the mirror and was immersed in an oil dashpot. The telescope and a millimetre scale were on a level with the mirror about 2 metres distant on one side of the balance. Two brass strips, parallel to each other and the beam, were fixed on the top of one arm of the beam, and in each of these were two **V** notches in which centigramme riders could rest. Two levers, worked by cams on a rod rotated by the observer, picked one rider up and let down the other, so that the effect was equivalent to the transfer of 1 centigrm. from one notch to the other. Their distance apart was such that this was equivalent to the addition of ·3284 milligrm. to one pan of the balance. This was the arrangement described in my former paper. Attached to one pan was a pair of brass strips parallel to each other, and such that the riders A, B, C, or D, would just rest across them. Two lifting rods worked up and down between these strips, so that of the two riders to be compared, one could be picked up at the instant the other was let down. The lifting rods were worked by a rod rotated by the observer and supported quite independently of the balance and of the slab on which it rested. By this plan the value of the scale divisions and the shifting of the centre of swing on changing the weights to be compared, could all be determined without raising the beam of the balance between the successive weighings, an essential condition, I believe, for exact work.

The weighings were made in the large room of the Physical Laboratory, and no precaution was taken to protect the balance case beyond placing a board in front of it. The room is draughty and subject to great variations of temperature, so that the weighings were made under very disadvantageous circumstances. One result of this was a rapid and sometimes very great change of resting-point in the course of a few hours, so that the scale passed out of the field of view. In order to bring it back without opening the case, two glass tubes passed through the top of the case, almost down to the scale pans, and small bits of wire could be dropped through these on to either pan as needed. Caps fitted on to the tubes to prevent draughts. This plan appears worthy of mention, as it suggests a mode of determining the value of a scale division when a balance is either too sensitive for riders or has no special arrangement for their accurate use. If a piece of wire weighing, say, 1 milligrm. is cut into say ten nearly equal parts, and if these are dropped on to the two pans alternately the shiftings of the centre of swing will be to and fro, about equal distances, due to about ·1 milligrm., but the sum of the shiftings will be that due to 1 milligrm., and the balance at the end will be nearly in the same position as at the beginning.

The following is an abstract of the comparisons of the riders. They were made soon after the first determinations of attraction on February 4, when A and D had not been dusted for three months.

In each case three extremities of swing were observed, and the centre of swing was determined from these by the graphic construction described later (p. 101).

The centres of swing were combined in consecutive threes in the usual way to give the differences in scale divisions.

Thus, in the first series, the successive centres of swing with D and A alternately in the scale pan were

D	A	D	A	D
231	223	217	211·9	208

whence

$$(D - A)_1 = \frac{217 + 231}{2} - 223 = + 1{\cdot}0 \text{ division.}$$

$$(D - A)_2 = 217 - \frac{223 + 211{\cdot}9}{2} = - 0{\cdot}45 \text{ division.}$$

$$(D - A)_3 = \frac{217 + 208}{2} - 211{\cdot}9 = + 0{\cdot}6 \text{ division.}$$

$$\text{Mean } D - A = {\cdot}38 \text{ division.}$$

Successive values of the differences alone are given below. The time of swing one way was about 16 seconds.

February 16, 1890.

(1.) Comparison of A and D, undusted.

Deflection due to ·328 *milligrm.* 83·45, 82·45, 84·45 divisions.
Mean 83·45 divisions.

D − A = 1·0, − ·45, + ·06 division. Mean ·38 division;

therefore D = A + ·0015 milligrm.

(2.) Comparison of A undusted, D dusted.

Value of scale division taken as in the last.

D − A = − ·5, + ·3, − ·1, − ·4, + ·25. Mean − ·09 division;

therefore D = A − ·0004 milligrm.

February 17, 1890.

(3.) Comparison of A and D, both dusted.

Value of scale division taken as below (4).

D − A = − ·1, − ·2, + ·3, − ·3, − ·8. Mean − ·22 division;

therefore D = A − ·0008 milligrm.

(4.) Comparison of C and D.

Deflection due to ·328 *milligrm.*, 85·35, 85·4, 84·65.
Mean 85·13 divisions.

D − C = + ·15, ·00, + ·05, − ·15, + ·05, + ·3, + ·05, − ·05, − ·35, ·05, ·35, ·45, ·50, ·5. Mean ·114 division;

therefore D = C + ·00044 milligrm.

February 18, 1890.

(5.) Comparison of C and D repeated.

Deflection due to ·328 *milligrm.*, 92·75, 92·3, 91·65.
Mean 92·23 divisions.

D − C = ·35, − ·05, − ·8, − ·95, − ·1, + ·05, 0, − ·15, − ·1, + ·05.
Mean − ·17 division;

therefore D = C − ·0006 milligrm.

Combining this with the last, and weighting them in the ratio of the numbers of determinations in each,

D = C + (·00044 × 14 − ·0006 × 10) ÷ 24 = C − ·0000 milligrm.

(6.) Comparison of A and D.

Value of scale division taken as above, ·328 milligrm. = 92·23 divisions.

D − A = ·45, ·25, ·1, − ·2, − ·1, ·35, ·25, ·45, ·60, ·5, ·5, ·55, ·5, ·75, ·55, ·1, ·05, ·45, ·10, ·30, ·8, ·9, ·35, ·2, ·30, ·55, ·50, ·35, ·45, ·45.

Mean ·378 division;

therefore D = A + ·00134 milligrm.

Examining the values obtained in (1), (2), and (3), it will be seen that no trustworthy evidence is given of a difference due to dusting. Any existing difference was probably under ·002 milligrm., and since the weighings on February 4, before dusting, were made with the attracted masses in the upper position, when the attraction was only one-fourth of that on which the final results depend, we may safely neglect the effect. After this the riders were dusted more frequently, so that we may probably assume their values more constant.

The comparisons of C and D, and of A and D, in (4), (5), and (6), were made more carefully. That of A and D in (6) is much the best of the series, the air in the laboratory happening to be steadier while it was made. The range between the greatest and least values of the difference is one scale division, or ·0036 milligrm., and the different results are grouped fairly closely about the mean.

The centres of swing and the differences are plotted in Diagram VIII. I do not claim that these results show any remarkable accuracy when compared with those obtained at the Bureau International des Poids et Mesures, but remembering how rough the apparatus was, and how little precaution was taken to ward off air currents, I have not the slightest doubt that, with special design of apparatus and more suitable locality, the results could be very greatly improved, and the accuracy carried far beyond anything hitherto reached. As they stand, they seem to show the value of the combination of a short time of swing with optical magnification.

The result of comparisons (4), (5), and (6), is, that if C has its Paris value, viz., C = 10·1196 milligrms., then, A = 10·1183 milligrms., and D = 10·1196 milligrms.; whence $\frac{1}{2}(A + D) = 10·119$ milligrms. This value may be used in calculating the result, since the riders were interchanged before Set II. was taken.

The losses experienced since 1886 by A and D are respectively, by A ·0064 milligrm., and by D ·0066 milligrm.—*i.e.*, they have diminished by practically equal amounts. This was to be expected, as they have probably received equal amounts of rough usage.

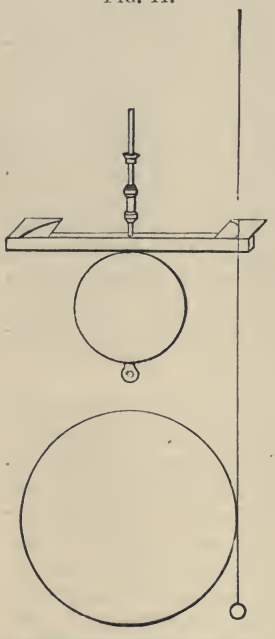
The substitution of the subsidiary beam for the cocoon fibre

suspension of the riders having greatly diminished the handling to which they were subjected, I have not thought it necessary to weigh them again during the work.

LINEAR MEASUREMENTS.

In the mathematical theory it will be shown that the lengths required are those marked in Fig. 17, viz., the horizontal distances, L and l, and the vertical distances, D_1 D_2, d_1 d_2, H_1 H_2, h_1 h_2.

FIG. 11.



Plumb-line adjustment of masses.

The Horizontal Distances.—Except when estimating the moment of the rider, the distance L is really that between the verticals through the centre of M and the centre of the more distant attracted mass. But the verticals through the centre of M in each position, so nearly passed through the centre of the mass above it, and, therefore, through the knife-edge from which it hung, that L was taken as equal to the length of the beam (p. 78).

The accuracy of this adjustment was secured as follows. A horizontal cross-piece was fixed on the top of each attracted mass, with two horizontal cards at its two ends, each with a portion of a circular arc on it, with radius equal to that of the large mass M, and with centre over that of the attracted mass (Fig. 11). A plumb-line was then hung just in front of the case, and the balance was moved by the horizontal screws bearing against the base plate until the plumb-line always appeared to touch the circular arc above, when it appeared to touch the large mass below. The adjustment was not quite perfect, but the error in the worst case was probably not more than 1 millim., and certainly less than 2 millims. Such an error in the horizontal distance is negligible.

The distance l had different values for the two positions occupied by m on the turntable. Calling these values l_1 and l_2 respectively, $l_1 + l_2$ was found by measuring a, the inside distance between M and m, arranged as in Set II., and b, the inside distance between them, when m was put on the same side of the turntable as M,

and adding to $a+b$ the sum of the diameters of M and m in the radial direction of the turntable as taken by square callipers.

The following are the values in terms of the cathetometer scale already referred to (p. 78), the temperature being 15° C. :—

a =	157·01
b =	33·95
Diameter of M =	30·52
„ „ m =	24·23
therefore $l_1 + l_2$ =	245·71

The value of $l_1 - l_2$ was found by measuring the shortest distance of m from the wall when respectively in the first and second positions on the turntable. It was found that

$$l_1 - l_2 = \cdot 12$$

whence

$$l_1 = 122{\cdot}915$$
$$l_2 = 122{\cdot}795$$

We may obtain from these measures an independent value of the radius of the circle in which the centre of M moves. With perfect adjustment this should be $\frac{1}{2}$L = 61·66 at 18°.

It is equal to a + radius of M + radius of m − l_2, or, by the above measures,

$$= 157{\cdot}01 + 15{\cdot}26 + 12{\cdot}115 - 122{\cdot}795$$
$$= 61{\cdot}59$$

which is only ·07 centim. less than $\frac{1}{2}$L.

Inasmuch as the wood probably expanded less than the cathetometer scale, while the metal expanded more, I have assumed as a rough approximation that the total expansion equalled that of the scale, so that the values of l_1 and l_2 are correct at 18°. No importance is, however, to be attached to this temperature correction.

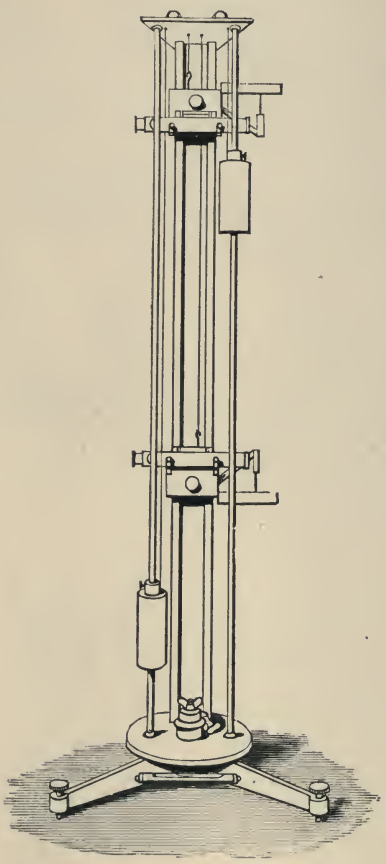
The Vertical Distances.—At the conclusion of each set of weighings with the attracted masses in a given position, the vertical distances between the top of the attracting masses and the bottom or top of the attracted masses (accordingly as they were in the upper or lower position) were measured by the cathetometer already referred to.

This instrument is of the well-known design of the Cambridge Scientific Instrument Company, and is especially adapted for measuring differences of level at different distances in different vertical planes. It reads to ·002 centim. The account of these measurements will be found in Table II. (p. 120 *et seq.*).

To find the distances D, d, H, h (Fig. 17), it was necessary to add to the actual distances measured the sum or difference of the vertical radii of the attracting and attracted masses, and, therefore, the vertical diameters of all the masses were measured.

For this purpose I used a cathetometer which has lately been constructed for me by Messrs. Bailey, of Bennett's Hill, Birmingham. I have to thank Mr. Potts, of that firm, for his care in its construction, and also for the trouble which he has taken in the construction and alteration of much of the apparatus used throughout the work recorded in this paper. As the cathetometer is, I believe, new in design and satisfactory in its performance, it appears worthy of description.

FIG. 12.



Cathetometer used to measure Vertical Diameters.

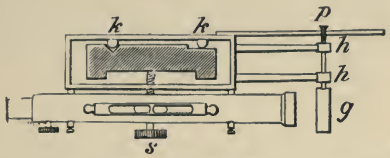
The Cathetometer used to measure Vertical Diameters (Fig. 12).—There are two telescopes, one to sight the upper, the other to sight the lower of the points between which the vertical height is required. There is no scale on the instrument, but after the telescopes are fixed to sight the two points the instrument is turned round a vertical axis, so that the telescopes sight a vertical scale at the same distance from them as the points. In general, of course, the cross wire will appear to lie between two divisions, but by means of the fine adjustment, to be described below, the two nearest scale divisions are brought in succession on to the cross wire, and by interpolation the reading corresponding to the point first sighted by the telescope is determined.

The telescopes are fixed on collars running up and down the main pillar, which has a section of the form shown in Fig. 13 (shaded).

The guides consist of three knobs, k k, on the inside of the collar, two sliding in a vertical **V**-groove and one on a plane, both groove and plane being at the back of the pillar. A screw, s, clamps the collar in any position. Gut strings running up over

pulleys and supporting counterpoises, sliding on the thinner pillars (see Fig. 12), are attached to the collars so that these move easily. At first springs were used to keep the knobs always in contact, but I find it much better to remove these and trust

FIG. 13.

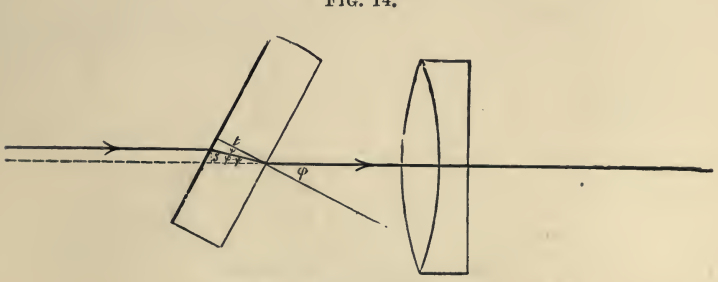


Section of pillar of new Cathetometer. *s*, clamping screw. *k*, *k*, guiding knobs. *g*, glass plate for fine adjustment, turning on axis *h*, *h*, with pointer at *p* perpendicular to plane of figure.

merely to hand pressure to keep the collars in their proper position before clamping with the screw *s*.

The fine adjustment is secured by the use of a piece of plate-glass placed in the front of each object glass* (*g*, Fig. 13), and capable of rotation about a horizontal axis, *h h*. A pointer is fixed on the end of this axis at *p*, and at its end is a small glass plate with a scratch on it moving close against a straight scale. If

FIG. 14.



Section of fine adjustment plate.

the plate is initially normal to the optic axis of the telescope, on turning it through ϕ, the ray which now comes along the optic axis has been shifted by transmission through the plate parallel to itself, a distance $t \sin(\phi - \psi) / \cos \psi$, where t is the thickness of the plate and ψ is the angle of refraction within it (see Fig. 14).

This shifting happens for small angles to be nearly proportional to tan ϕ, and, therefore, to the reading on the straight scale. To

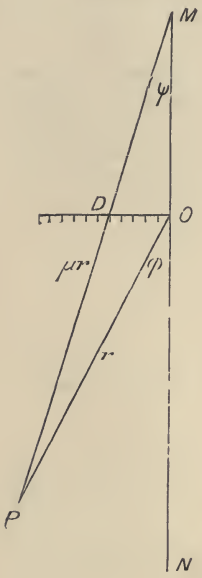
* Since this account of the adaptation of the parallel plate for fine adjustment was first published, I have found that it was used for micrometric work by Clausen in 1841, by Porro in 1842, and by Helmholtz in his Ophthalmometer. Porro used the plate both as an eyepiece micrometer and, as described in the text, in front of the object-glass ("Historical Note on the Parallel Plate Double-Image Micrometer," Monthly Notices R.A.S., liii. 5, p. 330).

show how nearly this is the case the following table gives the shifting for angles of 5°, 10°, and 20°, with a thickness of $t = 1$ centim. and a refractive index $\mu = \frac{3}{2}$:

Angle ϕ =	Shifting.
5°	$\frac{1}{3}$ tan 5° (1 − ·00042)
10°	$\frac{1}{3}$ tan 10° (1 − ·00156)
20°	$\frac{1}{3}$ tan 20° (1 − ·0052)

The error in taking the shifting as proportional to tan ϕ is, up to 20°, quite negligible in ordinary telescope-cathetometer work. If it is desirable to have greater accuracy, it is probably best to use a table of corrections to the tangent; but it is possible to get an exact scale thus:

FIG. 15.



Let OP, Fig. 15, represent the pointer of length r, making ϕ with a line MN. Let a pointer PM jointed to this at P be of length μr, and let its extremity M move on the line MN. Drawing OD at right angles to MN, if s is the shifting, we have

$$\mathrm{OD} = \mathrm{OP}\,\frac{\sin(\phi - \psi)}{\cos\psi}$$

$$= \frac{rs}{t},$$

$$\therefore\ s = \frac{t}{r}\,\mathrm{OD}.$$

Probably the practical difficulties in the use of such an arrangement would render it troublesome and uncertain.

The plate is used as follows: Adjust it normal to the optic axis of the telescope, and move the telescope till the required point is brought as near to the cross-wire as is possible by the hand. Clamp the telescope, and then turn the plate till the point is exactly on the cross-wire. Read the position of the pointer attached to the plate on its scale. Repeat these operations with the other telescope on the other point, then turn the instrument about its vertical axis till the telescopes sight the vertical scale placed at the same distance away as the two points. Looking through one of the telescopes the cross-wire is in general not exactly on a division. Turn the plate so that first the nearest division above, and next the nearest division below, is on the cross wire. Reading the position of the pointer in each case, inter-

polation gives us the reading on the vertical scale corresponding to the position of the pointer when the cross-wire was between the two scale divisions. Doing this for each telescope the difference between the two points is found in terms of the vertical scale.

The plates I have used are about 9 millims. thick, and the pointers about 9 centims. long. They move over scales such that 25 to 27 divisions correspond to a shifting of 1 millim. The lower scale is graduated from 0 to 50, the upper from 50 to 100 to prevent confusion. The 50 divisions occupy a distance of sixty-six millims.

It will be observed that in this form of instrument the level error is practically entirely obviated. It can only come in if the scale is not at the same distance as the height to be measured, and may then be made negligible in practice by levelling the telescopes. Indeed, the uncertainty of measurement appears only to depend on the uncertainty with which the cross-wire can be brought to the proper point, that is, it depends only on the magnifying power and definition of the telescopes used.

To illustrate the use of the instrument, a full account of the determinations of the vertical diameters is given in Table II. Below are the results, and for the sake of showing that there has certainly been no great change in shape, I give results obtained with a cathetometer more than ten years earlier at the Cavendish Laboratory at Cambridge.

—	1890.	1880.
	Centims.	Centims.
Large attracting mass M	30·526	30·5192
Small „ „ *m*	24·176	
Attracted mass A	15·8203	15·8166
„ „ B	15·7829	15·7842

The diameters of M and *m* in a horizontal direction parallel to a radius of the turntable measured by square callipers were

$$M = 30{\cdot}52 \text{ centims.}$$
$$m = 24{\cdot}23 \quad \text{„}$$

Temperature Correction.—Though the expansion of the masses was to be expected of an unimportant amount, I thought it advisable to attempt to measure it, in case there might be anything anomalous. One of the attracted spheres, B, was for this purpose placed between two vertical levers, in a tank through which could be run a continuous stream of cold or warm water. These levers depended from horizontal rods which could rock or slightly rotate on fine point suspensions. This was, in fact, a kind of double Lavoisier and Laplace apparatus. The motion of each lever was shown by another lever of about the same length, rising vertically

up from each horizontal axis, and serving as the moving support for a double suspension mirror in which was viewed the reflection of a millimetre scale. Two telescopes and one scale were used for the two mirrors, though it would not have been difficult to arrange one telescope and two scales. The value of one scale division was determined by inserting a piece of thin glass between the sphere and each lever in turn. The method is exceedingly sensitive, but I have not been able to make it exact, owing to the warping produced in the rods due to unequal temperatures.

The measures of the expansion varied between ·0000214 and ·0000277, both vertical and horizontal diameters (in the position in the balance) being tested. The true value is probably nearly ·000025 or 1/40000. It will, therefore, lead to no appreciable error if we take the expansion as equal to that of the scale of the cathetometer, say 1/60000(see Table II.).

Determination of the Attraction by the Balance.

When the balance is used to measure such small forces and weights as those with which we are here concerned, it must be left swinging on its knife-edge throughout any set of weighings in which the deflections are to be compared one with another. For there is not the slightest reason to suppose that if the beam is lifted up and let down again, its new position of equilibrium will coincide with the old. And again, the beam, especially with such loads as the attracted masses, is put into a state of considerable strain, and continues to alter its shape sensibly for hours, and, probably, even days, after the masses are put on to it. I have, therefore, always left the beam free for at least two or three days before commencing work with the balance, and it has of course remained free during the course of each day's work. The balance room was never entered just before any weighing, as it took many hours for the disturbance due to entrance and interference with the case to die away.

When the turntable supporting the attracting masses is moved half round, from one stop to the other, the bulk of the attraction is taken away from one attracted mass and put on to the other. The balance, being free, is slightly tilted over to the side on which is the larger attracting mass. But the deflection in the apparatus as arranged is so very small—at the most only ten scale divisions—that errors of reading can only be neutralised by making a great number of successive measures.

Probably other errors are also largely eliminated, such as those due to the deposition of dust particles, shaking, change of ground level, and varying air currents. Of such errors I have found those due to varying air currents by far the worst. Sometimes—especially

in autumn and winter—the balance will move quite irregularly through more than a scale division, and continue to move to and fro in this way for days or weeks. When in such an unsteady condition it is useless for accurate work. In spring and summer, however, it is much more steady as a rule, and frequently the scale can hardly be seen to move. I have never worked, when on looking into the telescope for some time the irregular movements appeared to be more than a fraction of a tenth—*i.e.*, a fraction of one of the diagonal divisions—though, doubtless, irregularities comparable with a tenth of a whole division have often made their appearance in the work. It is perhaps not safe to ascribe these always to air currents.

I have always found the air steadiest in warm quiet weather, with a slowly rising temperature in the balance room, and most unsteady after a sudden fall of temperature. As the alteration of temperature spreads downwards, this is fully in accord with Lord Rayleigh's observation that when the air is steady the ceiling is warmer than the floor, and that when it is unsteady the floor is the warmer of the two. In the observing room I had a gas stove often kept burning day and night, in the hope that the higher temperature it produced in the ceiling of the balance room below might steady the air. But the vertical walls of the balance room interfered with the action of the ceiling, and often produced unsteadiness.

A door opening or shutting anywhere in the building had a visible though transient effect, doubtless through an air wave. In a high wind the balance was always unsteady, partly, I suspect, through rushes of air into and out of the case with sudden pressure changes, and partly through changes of ground level, with variations of wind pressure against the building.

At all times there was a march in one direction or the other of the centre of swing. This was especially marked soon after the frame was lowered and the beam left free. As already remarked, readings were not taken till changes due to change in strain of the beam had subsided. But the march was very appreciable at other times, as will be seen from the diagrams. Perhaps the change was sometimes due to tilting of the ground, with barometric variation, since the balance was a very delicate level, and sometimes due to the change in buoyancy of the air affecting the two sides unequally, though I have not been able to make out any direct connection between barometric height and position of centre of swing. I believe that the explanation is to be sought for the most part in unsymmetrical effect on the beam of slight changes of temperature, for I have frequently noticed that a rising temperature produced an upward march, and a falling one a downward march. This explanation is supported by the following table of observations

of the centre of swing, extending from May 9 to May 22, 1890, the balance being free, and the balance room undisturbed meanwhile.

The relation between temperature and the centre of swing is represented in diagram IX.

Date, 1890.	Time.	Centre of Swing.	Temperature.		Barometer.
			Balance Room.	Observing Room.	
			°	°	
May 9 . .	11.5 A.M.	136·0	12·0	13.4	739·8
	12.55 P.M.	133·0	12·0	15·0	739·2
„ 12 . .	11.15 A.M.	133·8	12·05	14·5	738·6
	1.15 P.M.	131·9	12·05	15·8	738·3
	2.40 P.M.	133·7	12·05	16·6	738·1
	Stove left on all night of 12th–13th.				
„ 13 . .	11.0 A.M.	181·7	12·6	17·5	740·2
	12.35 P.M.	181·5	12·6	18·4	740·3
	3.15 P.M.	185·0	12·7	18·6	740·3
	Stove turned off.				
	5.25 P.M.	189·4	12·7	16·5	740·5
„ 14 . .	11.20 A.M.	167·4	12·6	14·3	745·8
	1.10 P.M.	165·5	12·6	14·4	746·0
„ 15 . .	11.5 A.M.	156·8	12·4	13·8	749·5
	2.45 P.M.	160·0	12·4	14·0	749·3
„ 16 . .	1.25 P.M.	158·3	12·4	14·0	744·3
„ 17 . .	8.50 P.M.	171·0	12·55	13·7	741·9
„ 19 . .	10.30 A.M.	174·3	12·55	14·0	743·5
	6.5 P.M.	181·8	12·6	14·0	741·0
„ 20 . .	11.30 A.M.	175·2	12·75	14·0	739·8
	1.5 P.M.	173·7	12·75	14·1	740·0
	5.20 P.M.	172·3	12·7	13·8	741·7
„ 21 . .	11.10 A.M.	172·7	12·6	13·7	751·9
„ 22 . .	11.30 A.M.	192 abt.	12·85	14·1	757·6

Of course, after a change in the position of the attracting masses or of the riders, the balance does not at once settle in a new position of equilibrium, but oscillates about it. Inasmuch as the balance never rests in this position, it is better to term it the centre of swing rather than the equilibrium position or resting point. The dashpot used to damp the vibrations of the mirror reflecting the scale serves also to damp those of the balance beam, and they die down rapidly. Instead of waiting, however, to observe directly the point on which they are closing in, it is much more exact, and also saves much time, to find the centre of swing as with an undamped balance from the extremities of the swings. I have always observed and recorded four extremities of three successive swings, occupying in all a little more than a minute.

Notwithstanding the very considerable damping, the successive lengths of swing are still in geometrical progression, but the rate

of reduction is too great to allow the ordinary approximation, in which the geometrical is assumed to be an arithmetical progression. The exact method of determining the centre of swing is as follows:

Let a, b, c, d be four successive readings of extremities of swing and let x be the reading of the required centre.

Let the constant ratio of each swing length to the next be λ.

Then

$$a - x = \lambda (x - b) \quad (1)$$

$$x - b = \lambda (c - x) \quad (2)$$

$$c - x = \lambda (x - a) \quad (3)$$

Eliminating λ from (1) and (2), we may readily obtain x in the form

$$x = c - \frac{(c - b)^2}{(a - b) + (c - b)} \quad (4)$$

and from (2) and (3)

$$x = b + \frac{(c - b)^2}{(c - b) + (c - d)} \quad (5)$$

With no disturbances and no errors of reading the values of x in (4) and (5) will coincide; but usually there is some small difference, the result of error or disturbance, and it is better to find both and take the mean. A third value might be obtained from (1) and (3); but it appears unadvisable to combine directly observations so far separated in time.

These formulæ lend themselves to easy arithmetical treatment, especially with the aid of a slide rule; but the following graphic method of finding the centre of swing is much less tiring and quite sufficiently exact.

Let the line OA, Fig. 16, represent the scale; O its zero, and A, B, C, D the points distant respectively a, b, c, d from O.

Let O′C′ be a parallel line, B′, C′, D′ being points opposite to B, C, D respectively. Let AB′ and BC′ intersect in K_1. Draw $X_1K_1X_1'$ perpendicular to OA. Then X_1 is the centre of swing given by equation (4). For

$$\frac{AX_1}{X_1B} = \frac{AX_1}{X_1'B'} = \frac{K_1X_1}{K_1X_1'} = \frac{X_1B}{X_1'C'} = \frac{X_1B}{X_1C},$$

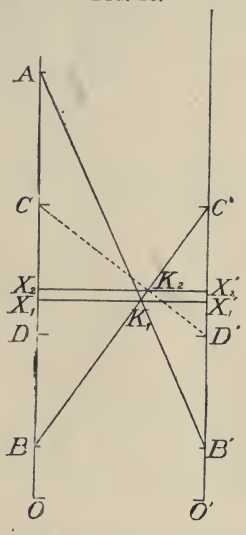
i.e., X_1 is the point dividing AB and BC in the same ratio. Similarly if BC′ and CD′ intersect in K_2, and $X_2K_2X_2'$ be drawn perpendicular to OA, X_2 is the point given by equation (5).

The third point given by equations (1) and (3) is obtained from the intersection of AB′and CD′, but evidently a small error in C or D′ may considerably alter the position of this point, and it is better not to use it.

The construction was carried out thus: a large opal glass plate, 10″ × 11″, was etched with cross lines 10 to the inch, so as to

present the appearance of ordinary section paper. The glaze was taken off so that pencil marks could be made. A diagonal line ran at 45° across the plate through the corners of the inch squares, and this was always taken as the line BC′ in the figure. Taking any convenient horizontal line, usually, of course, far below the plate, as zero, each inch represented a scale division, each tenth a diagonal division. The values of b and c fixed the lines to be taken as OA, O′C, and on these were marked the points A, C, B′, D′. A long glass slip, with a straight scratch on it, was then laid across from A to B′ so that the scratch passed through A and B′, and its intersection K_1 with the diagonal BC′ was x_1 from the zero line. The slip was then laid with the scratch passing through C and D′, and its intersection K_2 with BC′ gave x_2. It will be observed that all the actual construction for a set of readings of the balance swings consisted in marking four points on the plate.

FIG. 16.



The following cases, the first of very regular, the second of very disturbed swing, will serve to compare the results by this exact method with those obtained from the ordinary arithmetic mean method. At the same time they will show how nearly constant is the ratio of swing decrease.

Date and Number, 1890.	Scale Readings in Diagonal Divisions.	Length of Swing.	Ratio of Each to Preceding.	Centre of Swing, Exact.	Centre of Swing, Approximate $\frac{a+2b+c}{4}$.
May 4, No. 3	865 939 896 921	74 43 25	·581 ·581	911·8 911·8	909·75 913·0
				Mean 911·8	Mean 911·375
Sept. 17, No. 45	1118 1053 1093 1068	65 40 25	·615 ·625	1077·8 1078·6	1079·25 1076·75
				Mean 1078·2	Mean 1078·0

In finding the attraction the observations were always made in the same order, the determination of the scale value of rider and attraction being sandwiched so that each might be equally affected

by any comparatively slow changes. Starting with the initial position, the attracting masses and riders were so arranged that, on moving either, the balance was deflected in the same direction and over the same part of the scale.

The following was the order of proceeding always observed, the column headed "Centre of Swing" being supposed to contain the values of the position in each case determined from four swing extremities as just explained:—

	Centre of Swing.
(1) Initial position	i_1
(2) Riders moved	r_1
(3) Riders moved back to initial position	i_2
(4) Masses moved round	m_1
(5) Masses moved back to initial position	i_3
(6) Riders moved	r_2
(7) Riders moved back to initial position	i_4
(8) Masses moved round	m_2
and so on	

To minimise the effect of progressive changes these observations were always combined in threes in the following way. Denoting the scale value of rider by R, and of attraction by M:—

From (1), (2), (3) . . $R_1 = r_1 - \frac{i_1 + i_2}{2}$,

,, (3), (4), (5) . . $M_1 = m_1 - \frac{i_2 + i_3}{2}$,

,, (5), (6), (7) . . $R_2 = r_2 - \frac{i_3 + i_4}{2}$,

and so on.

These again were combined in threes, so that (the notation being continued) the successive values of attraction/rider are

$$\frac{2M_1}{R_1 + R_2}, \quad \frac{M_1 + M_2}{2}, \quad \frac{2M_2}{R_2 + R_3}, \quad \text{and so on.}$$

The successive centres of swing i_1, r_1, i_2, m_1, &c., correspond to instants of time following each other at intervals of about 2 minutes, rather more than 1 minute being taken up in making and recording the four readings for each, and the rest in making the change of position in rider or mass and waiting for the next readings. It will be seen that each value of M or R is based on three successive centres of swing, the weighings extending over about 6 minutes, while each value of M/R is based on seven successive centres of swing determined in about 14 minutes.

A series of readings was usually continued for about 2 or 3 hours. The temperature in both observing and balance room was

read at the beginning and end of the series, and the barometric height was also observed. As soon as possible after the desired number of determinations was completed with the attracted masses in one of the two positions, the vertical distances between attracting and attracted masses were measured by the cathetometer in the manner explained in Table II., and the position of the attracted masses was altered.

A full account of all the weighings is given in Table III., and the results are represented in Diagrams I.—VI. The three upper rows of points in each diagram represent the centres of swing, those in the initial position being marked ●. After movement of the rider they are marked ×, and after movement of the masses they are marked ○. The base lines for the different rows are altered to save space, as described on the diagrams, for on the scale adopted the rider series would always be about 10 inches above or below the initial series. In Diagram I. the rider and mass series are also brought down and superposed on the initial series, so that each of the three has the same average height. It will be seen that all three are affected by the same disturbances. The advantage of the short time of swing and the mode of combining the results in threes will be realised more easily from this superposition.

The base line may be regarded as a time scale, as the instants corresponding to successive centres of swing were almost exactly equidistant.

In each case, under the representation of the centres of swing, are plotted the resulting values of M/R and at the side will be found a representation of the distribution of results about the mean.

Assuming that each day's mean value is correct, and that the differences for different days are to be set down to variation of distance, &c., we can find the distribution of all the values about the mean by simply superposing the marginal curves at the side of the figures. The result fairly shows the accuracy as far as the weighing alone is concerned. It is represented in Diagram VII., where A is the mean value of the attraction in the lower, and a that in the upper position. A and a are brought near together to save space, but really they should be 40 inches apart. It will be seen that the range is about 2 per cent. of $A - a$ on each side of the mean, or taking the value of $A - a$ in milligrammes weight as about $\frac{1}{3}$ milligrm., and the load on each side as 20 kilogrms., the range is about $1/3 \times 10^9$. of this load on each side of the mean.

A comparison of the values of M/R in Diagrams I. and II., shows a very curious similarity in the fluctuations, and at first I was inclined to think there was some common external disturbance producing these fluctuations. But an analysis of the two sets of

values appeared to show that the resemblance is merely accidental. When the values of M and R are set out separately, it is seen that the fluctuations depend chiefly on M, of which the fluctuations are slightly like each other for the two series, while those of R are quite different, but such that they make the fluctuations in M/R resemble each other much more closely than those in M alone. Further, it is not easy to see how fluctuations due to some external source would affect the values of M equally in the upper and lower positions and not have any effect on R. Some periodic change of level might be suspected, but this ought certainly to be traced in R. I have examined all the other diagrams and plotted out the component values of M and R, but have found no trace of resemblance, so that I think the curious likeness in I. and II. must be set down to accident.

There is a curious step by step descent of the centre of swing in the initial position on September 23, Diagram VI., which I cannot explain. It may be due to some error in the method of finding the centre of swing which comes in with a rapid march of that centre. The effect on the result is probably only small, for the value of M/R obtained with a march in the reverse direction on September 25 is very nearly the same, the two values being

September 23 . . . . . ·2112753.
„ 25 . . . . . ·2112533.

The following is a list of the weighings recorded, with the distances measured and the mean values of the attraction:—

SET I.

Date. 1890.	Position of Attracted Masses.	No. of Values of M/R.	Mean Values of M/R.	D or *d* in Centims.	H or *h* in Centims.
Feb. 4	Upper	50	·2142212	62·318	61·416
April 30 and May 4 .	Lower	100	1·0109685	31·783	30·824
May 25	Upper	50	·2157379	62·308	61·373

SET II.

Date. 1890.	Position of Attracted Masses.	No. of Values of M/R.	Mean Values of M/R.	D or *d* in Centims.	H or *h* in Centims.
July 28	Lower	25	·9973168	32·106	30·965
Sept. 17	Lower	25	·9984148	32·116	30·954
Sept. 23 and 25 . .	Upper	52	·2112647	62·708	61·566

On the completion of Set I. the four masses were inverted, and changed over from right to left or left to right, and the initial position was after this always arranged so that movement of rider

or mass decreased the reading. This was done in order to lessen errors due to want of symmetry. If reversal had no effect, Set II. should, with the increased distance recorded above, give a value of M/R in the lower position of about ·990, instead of ·998. The larger value actually found is no doubt chiefly due to a want of symmetry in the large attracting mass M. The effect of this want of symmetry will be discussed after the investigation of the mathematical formula, and an account will be given of an independent method of detecting it. I think there is still outstanding a small difference, due, perhaps to want of symmetry in the turn-table or in the attracted masses. The result of the reversal shows how necessary it was to make it. I should have liked to have in Set II. as many determinations as in Set I., so that the mean should be based on values of equal weight. During June and July 1890, a complete set of 100 in each position, upper and lower, was made; but, owing to the pressure of other work, I was unable to calculate the results till the completion of the set. I then found that the value of M/R was still more than in Set II., and, on plotting out the results, it appeared that occasionally the rider value fell very considerably, and in an irregular way. On examination, there was little doubt that the rider came in contact occasionally with the suspending frame, when it was raised and should have been clear from it. Very likely temperature changes had brought about a displacement of the lever apparatus. Comparison with Set I. seemed to show that during that set no such contact had taken place, for there was no comparable irregularity. As it appeared dangerous to attempt to disentangle the good from the bad, the set of June and July was rejected, and Set II. was taken as recorded. When I had made the weighings giving 50 and 52 values in the two positions respectively, the balance became so irregular, through the cooler weather, that it was useless to continue work. Rather than carry over the experiment into another season, when it might be necessary to repeat the whole of the work, I have preferred to take Set II. as it stands, and give it the same weight as Set I. The final results are calculated from the means of Sets I. and II., as explained hereafter. I may here state the results obtained:—

Constant of attraction . . $G = \frac{6{\cdot}6984}{10^8}$.

Mean density of the earth . $\Delta = 5{\cdot}4934$.

General Remarks on the Method.

Comparing the common balance with the torsion balance, there is no doubt that the former labours under the great disadvantage that the disturbances due to air currents are greatest in the vertical

direction, that of the displacement to be measured. But even with this disadvantage the common balance may, I believe, be made to do much more than has hitherto been supposed possible. As an instrument in itself, apart from the external disturbances of air-currents, dust, &c., I believe its accuracy would be far beyond anything approached when these external disturbances are, as they always are, present to interfere with its action. I have always found that every precaution to ward off air currents and external disturbance has been accompanied by a corresponding increase in steadiness; and I have seen no sign of a limit of accuracy depending on the instrument itself.

Besides the protection from air currents, there are two conditions essential above all others for accurate work:—

1st. That during any set of weighings in which the deflections are to be compared with each other, the beam should be supported on its knife edge, and should be under constant strain.

2nd. That all moving parts, such as apparatus for changing riders or weights, should be supported quite independently of the balance or its case.

With regard to the first condition, it seems impossible to make the supporting frame move so truly and with so little disturbance that the knife edge shall return exactly to the same line. Even were it possible, the beam after raising and lowering would be practically a different beam, for, as my observations show, the condition of strain changes considerably after the load is first put on, and it would be merely a chance coincidence if the mean state of strain were the same during successive weighings. In the "Proceedings of the Royal Society," No. 190, 1878, one method of comparing weights of nearly equal value with the beam throughout on its knife-edge and equally strained is described,* and I should now only modify that method in having regard to the second condition, of which I have since realised the importance when working with the large balance and with increased optical sensitiveness. It is surprising to find how much disturbance is produced by having the moving parts of the apparatus connected with the balance or its case.

As to air currents there is no doubt that, as Professor Boys has shown, the greater the apparatus the greater the errors produced by them. At the time my apparatus was designed I did not know this, and there seemed to be a great advantage in making it large, as riders could be used of weight large enough to be measured accurately. Were I about to start with a new design I should certainly go towards the other extreme and make the apparatus

* I am glad that Dr. Thiesen urges the importance of this condition ("Travaux et Mémoires du Bureau International des Poids et Mesures,' vol. v., "Etudes sur la Balance").

small, attempting to get over the rider difficulty by some such method as that explained on p. 89. For not only is a smaller apparatus kept more easily at a uniform temperature, and, therefore, freer from the source of air currents, but it is much more handy to adjust, and even if the adjustments are not more accurate they will at least take much less time to make.

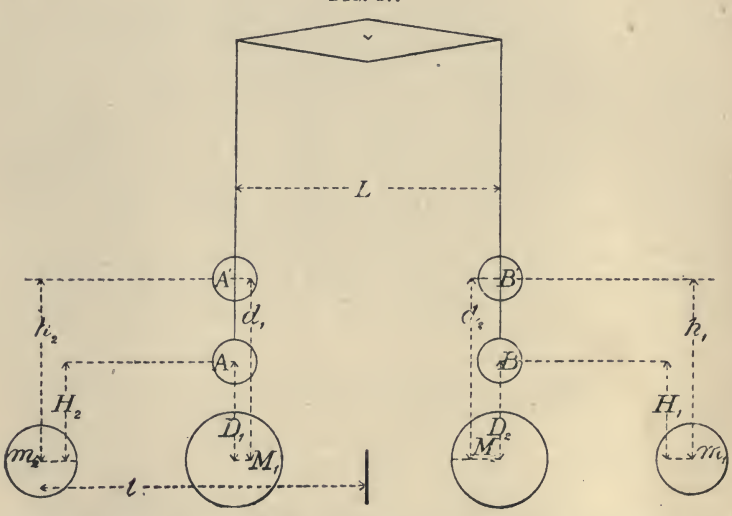
At the same time it is only fair to say, on behalf of the large apparatus, that some errors have been magnified on a like scale till they have become observable, and so could be investigated and eliminated. Starting with a small apparatus they would probably never have been detected, and would, therefore, have appeared in the final result.

II. MATHEMATICAL INVESTIGATION.

The Value of the Attraction Expressed in Terms of the Masses and Distances, and the Investigation of the Effect of Want of Symmetry in the Masses.

Let us suppose that initially the attracting masses are in the positions M_1, m_1, Fig. 17, the larger on the left, the smaller on the

FIG. 17.



right, and that the attracted masses are in the lower position A, B. When the turntable is moved round so that the positions of the masses are M_2, m_2, the greater attraction is taken from the left and put on to the right. Let the centre of swing of the balance

alter by an amount corresponding to a total change of vertical pull of n dynes. Assuming that a spherical mass M attracts another spherical mass M′ when their centres are D centimetres apart with a force of GMM′/D² dynes, we can express the change of vertical pull due to the change of position of the masses as G × a function F of the masses and distances. There is also a change of pull on the suspending rods and the balance beam which we may denote by E.

Then $$n = GF + E.$$

In order to eliminate E let the attracted masses be moved into their upper positions A′, B′, and let the change on moving round the attracting masses be n' dynes. If f is the function of the masses and new distances corresponding to F,

$$n' = Gf + E.$$

Subtracting $$n - n' = G(F - f),$$

whence $$G = \frac{n - n}{F - f},$$

and knowing G, the mean density of the earth may be at once found in the manner shown later.

We have then to find the form of the functions, F, f, and as a preliminary step it is necessary to find the effect of the holes bored through the attracted masses A,B. This may be made to take the form of a correcting factor to the attraction which would be exercised on them if they were spheres.

The piece bored out in each case has radius ·31 centim. This we denote by c. It may be taken as practically a cylinder with plane ends and length equal to 15·8 centims., the diameter $2r$ of the spheres. The intensity due to such a cylinder of mass μ at D from its centre is (Todhunter's "An. Stat.," 5th ed., p. 293),

$$G\mu \frac{2r - \sqrt{\{(D + r)^2 + c^2\}} + \sqrt{\{(D - r)^2 + c^2\}}}{c^2 r},$$

which equals, to a sufficient approximation,

$$\frac{G\mu}{D^2 - r^2}.$$

If the mass remaining after μ is removed is A, and if the centre of the mass M is D below that of A, the attraction of M on A is

$$\begin{aligned} &\frac{GM(A + \mu)}{D^2} - \frac{GM\mu}{D^2 - r^2} \\ &= \frac{GMA}{D^2} + GM\mu\left(\frac{1}{D} - \frac{1}{D^2 - r^2}\right) \\ &= \frac{GMA}{D^2}\left\{1 - \frac{\mu}{A}\left(\frac{r^2}{D^2} + \text{higher powers of } \frac{r^2}{D^2}\right)\right\}. \end{aligned}$$

Now

$$\frac{\mu}{A} = \frac{2\pi c^2 r}{\frac{4}{3}\pi r^2 - 2\pi c^2 r} = \frac{\frac{3}{2}c^2}{r^2} \text{ nearly } = \tfrac{3}{2}(\tfrac{31}{700})^2 = \cdot 00231,$$

and the greatest value of

$$\frac{r^2}{D^2} = (\tfrac{7}{3}\tfrac{9}{2}\tfrac{}{0})^2 = \cdot 061.$$

Then the higher powers may be neglected, and the attraction may be written

$$\frac{GMA}{D^2}\left(1 - \frac{\frac{3}{2}c^2}{D^2}\right) = \frac{GMA}{D^2}(1 - \theta), \text{ say.}$$

When A and B are in the lower position, D = 32, and $1 - \theta =$ ·99986. When they are in the upper position, D = 62 and $1 - \theta =$ ·99996, a value so near 1 that we shall in this position omit the correction, since it is only applied to one-fourth of the final result.

In the cross attractions we shall also omit the correction.

Referring to Fig. 1 let the vertical differences of level between the centres of the various spheres be denoted as follows, the suffixes to M and m denoting their first and second positions respectively:—

$A - M_1 = D_1$	$B - M_1 = D_1'$,
$B - M_2 = D_2$	$A - M_2 = D_2'$,
$B - m_1 = H_1$	$A - m_1 = H_1'$,
$A - m_2 = H_2$	$B - m_2 = H_2'$.

When the masses A, B, are placed in their upper positions, let the corresponding distances be denoted by small letters.

Let the horizontal distance between the centres of A and B be L, being within sensible limits equal to that between the centres of M in its two positions, and to the length of the beam, and let the radius of the circle in which m moves be l.

Then we have the following horizontal distances:—

$$A - M_2 = B - M_1 = L,$$
$$A - m_1 = B - m_2 = l + \tfrac{1}{2}L,$$
$$A - m_2 = B - m_1 = l - \tfrac{1}{2}L.$$

We may now write the change in vertical pull on the left by the motion of M from left to right, and of m from right to left, as follows—the first four terms representing the vertical attractions on A and B by M and m in their first position, the next four their attractions when moved round, and the last term E representing the change in attraction on the beam and suspending rods:—

$$G\left\{\frac{MA(1-\theta)}{D_1^2} - \frac{MBD_1'}{(D_1'^2+L^2)^{\frac{3}{2}}} - \frac{mBH_1}{\left\{H_1^2+\left(1-\frac{L}{2}\right)^2\right\}^{\frac{3}{2}}}\right.$$

$$+ \frac{mAH_1'}{\left\{H_1'^2+\left(l+\frac{L}{2}\right)^2\right\}^{\frac{3}{2}}} + \frac{MB(1-\theta)}{D_2^2} - \frac{MAD_2'}{(D_2'^2+L^2)^{\frac{3}{2}}}$$

$$\left. - \frac{mAH_2}{\left\{H_2^2+\left(l-\frac{L}{2}\right)^2\right\}^{\frac{3}{2}}} + \frac{mBH_2'}{\left\{H_2'^2+\left(l-\frac{L}{2}\right)^2\right\}^{\frac{3}{2}}}\right\} + E.$$

We may arrange all but the last term in nearly equal pairs.

Thus the first and fifth go together, and if we put $D_1+D_2=2D$, and $D_1+\delta=D_2-\delta=D$, their sum is

$$GM(1-\theta)\left(\frac{A}{D_1^2}+\frac{B}{D_2^2}\right)$$

$$= GM(1-\theta\left\{\frac{A}{D^2}\left(1+\frac{2\delta}{D}+\frac{3\delta^2}{D^2}+\ldots\right)\right.$$

$$\left.+\frac{B}{D^2}\left(1-\frac{2\delta}{D}+\frac{3\delta^2}{D^2}-\ldots\right)\right\}$$

$$= GM(1-\theta)\frac{A+B}{D^2}\left\{\left(1+\frac{3\delta^2}{D^2}+\text{higher powers of }\frac{\delta^2}{D^2}\right)\right.$$

$$\left.+\frac{A-B}{A+B}\cdot\frac{\delta}{D}\left(2+\frac{4\delta^2}{D^2}+\ldots\right)\right\}.$$

Now $(\delta/D)^2$ is negligible, as will be seen by reference to the table of distances, and $(A-B)/(A+B)$ is less than $4/3000$, or of the same order as δ/D.

To a sufficiently close approximation, then, the sum of the two terms is

$$\frac{GM(A+B)(1-\theta)}{D^2}.$$

The second and sixth terms may also be taken together, and putting

$$D_1'+D_2'=2D' \text{ and } D_1'+\delta'=D_2'-\delta'=D',$$

we may show that to a sufficient approximation

$$GM\left\{\frac{BD_1'}{(D_1'^2+L^2)^{\frac{3}{2}}}+\frac{AD_2'}{(D_2'^2+L^2)^{\frac{3}{2}}}\right\} = \frac{GM(A+B)D'}{(D'^2+L^2)^{\frac{3}{2}}}.$$

The two pairs with m give similar results with

$$H=\tfrac{1}{2}(H_1+H_2) \text{ and } H'=\tfrac{1}{2}(H_1'+H_2').$$

Now $$2D = D_1 + D_2 = A - M_1 + B - M_2$$
$$= B - M_1 + A - M_2 = D_1' + D_2' = 2D',$$

and similarly $2H = 2H'$, so that we may put the expression in the form

$$G\left\{\frac{M(A+B)(1-\theta)}{D^2} - \frac{M(A+B)D}{(D^2+L^2)^{\frac{3}{2}}} - \frac{m(A+B)H}{\left\{H^2+\left(l-\frac{L}{2}\right)^2\right\}^{\frac{3}{2}}}\right.$$
$$\left. + \frac{m(A+B)H}{\left\{H^2+\left(l+\frac{L}{2}\right)^2\right\}^{\frac{3}{2}}}\right\} + E = GF + E \text{ say.}$$

It is evident that we may combine experiments at different distances on different occasions in the same way by taking D and H to represent the mean values of these distances, so long as there is only a small variation from the mean.

If the attracted masses are now moved into their upper positions the expression for the change in attraction may be at once deduced from that in the lower position by replacing D and H by d and h, and omitting the factor $1-\theta$. Let it be denoted by $Gf+E$.

Subtracting one expression from the other E is eliminated, and we have

$$G(F-f)$$
$$= G\left\{\frac{M(A+B)(1-\theta)}{D^2} - \frac{M(A+B)D}{(D^2+L^2)^{\frac{3}{2}}} - \frac{m(A+B)H}{\left\{H^2+\left(l-\frac{L}{2}\right)^2\right\}^{\frac{3}{2}}}\right.$$
$$+ \frac{m(A+B)H}{\left\{H^2+\left(l+\frac{L}{2}\right)^2\right\}^{\frac{3}{2}}} - \frac{M(A+B)}{d^2} + \frac{M(A+B)d}{(d^2+L^2)^{\frac{3}{2}}}$$
$$\left. + \frac{m(A+B)h}{\left\{h^2+\left(l-\frac{L}{2}\right)^2\right\}^{\frac{3}{2}}} - \frac{m(A+B)h}{\left\{h^2+\left(l+\frac{L}{2}\right)^2\right\}^{\frac{3}{2}}}\right\}.$$

This is to be equated to the difference in the values of the change in attraction in the two positions, as determined by the rider.

Let

b = the length of the small rider beam.
w = the mass of each rider.
A = mass deflection ÷ rider deflection in lower position.
a = " " " " " upper "
g_B = acceleration of gravity, or dynes weight per unit mass at Birmingham.

Then $$G(F-f) = \frac{(A-a)\,bwg}{\frac{1}{2}L}.$$

Whence we may find the gravitation constant

$$G = \frac{2bwg_B(A - a)}{L(-f)},$$

where all the quantities on the right hand are given in the Tables at the end.

The value of g_B may be found sufficiently nearly from the formula (Everett's "Units," p. 21); $g_B = 980{\cdot}6056 - 2{\cdot}5028 \cos 2\lambda - {\cdot}000003h$, where λ is the latitude $= 52° 28'$ at Birmingham, and h is the height above sea-level, which may be taken as 450 feet, or 13,725 centims. Whence $g_B = 981{\cdot}21$.

Since all the operations are conducted in air, the effective masses should throughout be less by the mass of air each displaces. But since they all have nearly the same densities, and w and $A + B$ appear respectively in numerator and denominator, it is sufficient to take their true masses, and to correct for air displaced in the case of M and m only.

To obtain the mean density of the earth Δ, we must express the acceleration of gravity in terms of G and the mass and dimensions of the earth.

The ordinary formula (Pratt, "Figure of the Earth," 4th ed., p. 119) is based on the assumption that the earth is a spheroid. It is sufficiently correct for our purpose, the departure of the assumed spheroid from the actual shape being very small. Adding a term $-3 \times 10^{-9}h$, or approximately, -41×10^{-6}, since the balance room is taken as 13,725 centims. above sea-level (see above), the value of gravity at Birmingham may be written

$$g_B = \frac{GV\Delta}{a^2}\left\{1 + \frac{\varepsilon}{3} - \frac{3}{2}m + \left(\frac{5}{2}m - \varepsilon\right)\sin^2 52° 28' - 41 \times 10^{-6}\right\},$$

where

V = volume of the earth $= 1{\cdot}0832 \times 10^{27}$ (Everett's "Units," p. 57).
a = mean radius of the earth $= 6{\cdot}3709 \times 10^8$ (*loc. cit.*).
Δ = mean density of the earth.
m = equatorial "centrifugal force" ÷ gravity $= \frac{1}{289}$.
ε = ellipticity of the earth $= \frac{1}{282}$.

The value of the ellipticity is taken to make the formula agree with that quoted above from Everett's "Units." The uncertainty in the value is quite unimportant, for were ε as low as $\frac{1}{296}$, the error in Δ, introduced by taking it as $\frac{1}{282}$, would be less than 1 in 50,000.

Substituting for G, the value of the mean density of the earth is

$$\Delta = \frac{a^2 L(F-f)}{2bwV\left\{1+\frac{\epsilon}{3}-\frac{3}{2}m+\left(\frac{5}{2}m-\epsilon\right)\sin^2 52^\circ 28' - 41\times 10^{-6}\right\}\left(A-a\right)}.$$

Here, as in the value of G, w and $A+B$ may have their true values, M and m their values less the mass of air displaced.

In the foregoing investigation we have supposed that all the masses are homogeneous and spherical, with the exception of the borings through A and B. We have supposed, also, that the turntable is exactly symmetrical about a vertical plane through its axis, so that its motion through two right angles is without effect. Doubtless these suppositions and the formula based on them are not quite true. But, if we invert all the masses and change their sides, or pervert the whole arrangement of them, on taking the mean of the results obtained in the original and inverted and perverted positions we ought to greatly reduce the errors. Indeed, those due to want of symmetry in the turntable should evidently be quite eliminated, and those due to want of homogeneity in the masses should certainly be lessened.

To show this, we shall calculate the effect of a spherical "blowhole," or gas cavity in M, in the first and most important term of F. This we shall take as being

$$\frac{GM\,(A+B)}{D^2},$$

on the supposition that M is homogeneous and spherical.

If the mass of metal which would fill the blowhole is λ, supposing it to be placed there, the sphere is completed and its attraction is

$$\frac{G\,(M+\lambda)\,(A+B)}{D^2}.$$

But the vertical attraction is less than this in reality by the vertical component of the attraction of λ.

Let

B be the centre of the cavity,
P the centre of the attracted mass,
O the centre of the attracting mass,
δ the distance of B from the centre of M,
θ the angle BOP.

The vertical component of the attraction of λ is

$$\frac{G\lambda\,(A+B)\cos BPO}{PB^2},$$

but $$BP^2 = D^2 + \delta^2 - 2D\delta\cos\theta,$$

and $$\cos \text{BPO} = \frac{\text{D} - \delta \cos \theta}{\text{BP}},$$

whence the attraction of λ may be put

$$\frac{G\lambda\,(A + B)\,(D - \delta \cos \theta)}{D^2 + \delta^2 - 2D\delta \cos \theta^{\frac{3}{2}}} = -G\lambda\,(A + B)\,\frac{d}{dD}\,\frac{1}{\sqrt{D^2 + \delta^2 - 2D\delta \cos \theta}},$$

$$= \frac{G\lambda\,(A + B)}{D^2}\left(1 + 2P_1\,\frac{\delta}{D} + 3P_2\,\frac{\delta^2}{D^2} + \ldots\right),$$

where P_1, P_2 . . . are zonal harmonics. The attraction of the sphere with the cavity is therefore

$$\frac{GM\,(A + B}{D^2}\left\{1 - \frac{\lambda}{M}\left(2P_1\,\frac{\delta}{D} + 3P_2\,\frac{\delta^2}{D^2} + 4P_3\,\frac{\delta^3}{D^3} + \ldots\right)\right\}.$$

If the mass is inverted, the vertical component is obtained by changing the sign of δ, and the mean of the two values is

$$\frac{GM\,(A - B)}{D^2}\left\{1 + \frac{\lambda}{M}\left(3P_2\,\frac{\delta^2}{D^2} + 5P_4\,\frac{\delta^4}{D^4} + \ldots\right)\right\},$$

the first power of δ / D being eliminated.

If $\theta = 0$, P_2 and all the other harmonics $= 1$.

If $\theta = 90°$, $P_2 = -\frac{1}{2}$, $P_4 = \frac{3}{8}$, &c.

Now, with the actual dimensions of the apparatus, $(\delta/D)^2$ cannot be so great as $(\frac{1}{2})^2$ or $\frac{1}{4}$, and may, of course, be much smaller. The first term of those involving λ, therefore, is the most important, and it lies between $+\frac{3}{2}\,(\lambda/M)\,(\delta^2/D^2)$ and $-3\,(\lambda/M)\,(\delta^2 D^2)$ changing sign for the value of θ given by $P_2 = 0$.

The second set of experiments recorded in this paper was taken after inversion and change of side of all the masses, and the final result obtained from this set differs by a little more than 1 per cent. from that obtained from the first set, the observed attraction being slightly greater at the same distance. The difference may be due to irregularities in any or all of the masses and in the turntable, and to other undetected effects, such as change of level on rotating the turntable. It would be a very long task to disentangle these, and I have contented myself with trying to find how much must be set down to irregularity in the large mass M, by taking a set of weighings with it alone inverted.

After the weighings on July 28, and the subsequent measures of distances, M was inverted only, and the other masses remained as in Set II. Some weeks later on, September 14, 25 values of $M/R = A$ were obtained, the mean being ·9926. The distances were $D = 32{\cdot}118$, $H = 30{\cdot}978$. The mass M was then put in its original position, as in Set II., and on September 17, on referring

to the tables, it will be seen that the value of M/R obtained was ·9984, the distances being D = 32·117 and H = 30·955, practically the same as on September 14.

Assuming that the difference in attraction is due to cavities in various places, and that, for each, the term $3P_2\delta^2/D^2$ is negligible, we have, approximately,

$$\frac{1 - 2\dfrac{\Sigma\lambda P_1\delta}{MD}}{1 - 2\dfrac{\Sigma\lambda P_1\delta}{MD}} = \frac{9926}{9984}.$$

Whence, approximately, since D = 32.

$$\frac{\Sigma\lambda P_1\delta}{M} = \cdot 0464 \text{ centim.}$$

This result may be tested by independent experiment. For let the centre of gravity be x below the horizontal plane through the point bisecting the vertical diameter (*i.e.*, the centre of figure), in the position of September 14. The distance of any missing particle λ from the horizontal plane is $\delta \cos\theta = P_1\delta$. Completing the sphere by the addition of all such particles, the centre of gravity is brought to the centre of figure, so that we have

$$Mx = \Sigma\lambda P_1\delta,$$

and

$$x = \frac{\Sigma\lambda P_1\delta}{D}.$$

We have, therefore, to determine the vertical distance of the centre of gravity from the centre of figure.

In order to do this, a large flat-bottomed scale-pan (one belonging to the balance used in the gravitation experiment) was suspended by two parallel wires about 8 centims. apart and 3 metres long. In the middle of the pan was a shallow cup about 7·5 centims. internal diameter, arranged so that it could turn freely but truly about a vertical axis. The mass, M, was placed on this cup with the diameter, which had been vertical, arranged horizontal, and perpendicular to the plane of the suspending wires. A vertical flat plate, worked by a horizontal micrometer screw, could be brought just in contact with the end of the diameter, and the reading of the micrometer gave the position of the point of contact. The position of the scale-pan was determined by a plumb-line hanging over one edge in front of a horizontal scale. On turning the cup and mass through 180°, and repeating the readings, knowing the weight of the scale-pan

and the position of its centre of gravity, x could at once be found.

Two separate experiments gave

$$x = \cdot0536 \text{ centim.},$$

and

$$x = \cdot0516 \text{ centim.},$$

not very different from the value ·0464, obtained from the attraction experiments. The agreement is, I think, very close when it is noted that a difference in the attraction in one of the sets of weighings of 1 in 1000 would make x either ·038 or ·054.

This result appears to justify the rejection of all terms in the expansion above the first, and so supports the belief that the reversal largely eliminates errors due to irregularities of shape. For it is in the case of M that there is the greatest danger of large value for δ/D, and the above experiments seem to indicate that even in this case it is small.

It is, perhaps, noteworthy that the largest term rejected in the attraction of M, viz., $3\lambda P_2\delta^2/MD^2$ is, if we give P_2 its maximum value 1,

$$\frac{3\lambda\delta}{MD} \cdot \frac{\delta}{D} = \frac{3x}{D} \cdot \frac{\delta}{D},$$

which is not greater than

$$\frac{3 \times \cdot0464}{32} \times \frac{15}{32} = \cdot0020,$$

since the radius of the mass is 15.

This is in a term about 5/4 of the final result, so that the greatest error which can be introduced by neglecting this term is ·0025, or 1 in 400.

In calculating the results of the experiments the means of Sets I. and II. have been taken. Equal weights have been given to each set. It would have been more satisfactory if the number of experiments had been the same in each set; but I should have had to wait for another season to obtain more, and then it would, probably, have been necessary to repeat the whole series in both arrangements, as it is not safe to assume that the various disturbing causes remain the same over a wide interval of time. The second set, though fewer in number, are, in some respects, I believe, better; partly owing to the additional experience gained when they were taken.

In order that the various terms in $F - f$ may be compared, I give below their numerical values, as determined from the values of the masses and distances given in the tables. The meaning of

each term in the first column will be seen on referring to Fig. 17. The second column contains the actual values; the third column the values in terms of the fourth, the lowest term.

	VALUE of F − *f*.	
$\frac{M(A+B)(1-\theta)}{D^2}$	= 6483938·8	416
$-\frac{M(A+B)D}{(D^2+L^2)^{\frac{3}{2}}}$	− 102416·3	6·6
$-\frac{m(A+B)H}{\left\{H^2+\left(l-\frac{L}{2}\right)^2\right\}^{\frac{3}{2}}}$	− 316243·3	20
$+\frac{m(A+B)H}{\left\{H^2+\left(l+\frac{L}{2}\right)^2\right\}^{\frac{3}{2}}}$	+ 15579·9	1
$-\frac{M(A+B)}{d^2}$	− 1693687·2	109
$+\frac{M(A+B)d}{(d^2+L^2)^{\frac{3}{2}}}$	+ 156728·0	10
$+\frac{m(A+B)h}{\left\{h^2+\left(l-\frac{L}{2}\right)^2\right\}^{\frac{3}{2}}}$	+ 310695·0	20
$-\frac{m(A+B)h}{\left\{h^2+\left(l+\frac{L}{2}\right)^2\right\}^{\frac{3}{2}}}$	− 27597·7	1·7

Whence F − *f* = 4826997·2.

The mean value of A − *a* (see Table III.), is

$$A - a = \cdot 791295;$$

substituting these values of F − *f* and A − *a* in the formula for G (p. 113), we obtain

$$G = \frac{6 \cdot 6984}{10^8};$$

substituting them in the formula for Δ we obtain

$$\Delta = 5 \cdot 4934.$$

The values given by Sets I. and II., treated separately, are to two figures of decimals:

Set I. Δ = 5·52
Set II. Δ = 5·46.

III. TABLES.

Table I.—Constants of the Apparatus and Dimensions of the Earth.

Masses.

	Grms.
Attracting mass M, in vacuo . . .	= 153407·26
Less air displaced, say	= 153388·85
Attracting mass m, in vacuo . . .	= 76497·4
Less air displaced, say	= 76488·2
Attracted mass A, in vacuo . . .	= 21582·33
,, ,, B, ,, . . .	= 21566·21
,, ,, A + B, in vacuo .	= 43148·54
Riders each, in vacuo	= 0·010119

Vertical Diameters of Masses in Terms of Cathetometer Scale correct at 18°.

The masses are taken as having the same coefficient of expansion as the scale.

	Centims.
M	= 30·526
m	= 24·176
A	= 15·8203
B	= 15·7829.

The diameters of the masses A and B are taken between the nuts securing them on the suspending wires.

	Centims.
Balance beam at 0°, L	= 123·232
Rider beam at 0°, b	= 2·53575
L/b (as occurring explicitly in G and Δ, independent of temperature, assuming them to have the same coefficient of expansion)	= 48·59775

Latitude of Birmingham	= 52° 28′
Height of balance room above sea-level	= 13725 centims.
Gravity at Birmingham, g_B . . .	= 981·21 centims./sec.^{2}
Mean radius of earth	= 6·3709 × 10^8 centims.
Volume of earth	= 1·0832 × 10^{27} c. centims
Equatorial "centrifugal force"/gravity	= $\frac{1}{289}$
Ellipticity of earth	= $\frac{1}{282}$
$1 + \frac{\epsilon}{3} - \frac{3m}{2} + \left(\frac{5}{2}m - 2\right)\sin^2 52° 28′ - 41 \times 10^{-6}$	= ·999161.

Table II.—Vertical and Horizontal Distances.

Vertical Diameters of Masses taken by the Cathetometer (described p. 94).

In the tables below, P.S. signifies divisions on the scale over which moves the pointer, which is attached to the small adjustment plate. V.S. signifies divisions on the vertical millimetre scale.

Diameter of Large Attracting Mass M.

	Reading on pointer scale.	Mean.
Upper telescope sighting top of mass . .	73·2, 73·4, 73·2	73·27 P.S.
Lower telescope sighting bottom of mass .	23·0, 23·0, 23·2	23·07 P.S.

Turning round to the Vertical Scale.

	Reading on pointer scale.	Mean.
Upper telescope sighting 459 millims. V.S.	94·6, 94·9, 94·4, 95·0, 94·0	94·58 P.S.
Upper telescope sighting 458 millims. V.S.	68·8, 68·8, 69·7, 69·0, 68·7, 70·0, 69·6, 69·4	69·25 P.S.

Therefore 25·33 P.S. divisions = 1 millim. V.S.,

and scale reading for top of mass $= 458 + \frac{73{\cdot}27 - 69{\cdot}25}{25{\cdot}33}$

$= 458{\cdot}158$ millims. V.S.

	Reading on pointer scale.	Mean.
Lower telescope sighting 153 millims. V.S.	27·3, 27·4, 27·7, 27·0	27·35 P.S.
Lower telescope sighting 152 millims. V.S.	0·0, 0·3, − 0·5, 0·0	− 0·07 P.S.

Therefore 27·42 P.S. divisions = 1 millim. V.S.,

and scale reading for bottom of mass $= 152 + \frac{23{\cdot}07 + 0{\cdot}07}{27{\cdot}42}$

$= 152{\cdot}844$ millims. V.S.

The difference = 30·5314 centims.

This is rather greater than the diameter of the mass, as the cross wire was made to touch the image of the mass in each case. A series of measures of 1 millim. on the scale, in which the cross wire was on the centre of each division, and of 1 millim. between the jaws of a wire gauge, in which the wire touched the images of the jaws, showed that at the distance at which the scale was, ·005 centim. must be subtracted, leaving

Diameter of M = 30·526 centims.

VERTICAL Diameter of Small Attracting Mass m.

	Reading on pointer scale.	Mean.
Upper telescope sighting top of mass . .	75·6, 75·6, 75·0	75·40 P.S.
Lower telescope sighting bottom of mass .	26·5, 26·3, 26·8	26·53 P.S.

TURNING round to the Vertical Scale.

	Reading on pointer scale.	Mean.
Upper telescope sighting 388 millims. V.S.	100, 99·9, 99·7	99·87 P.S.
Upper telescope sighting 387 millims. V.S.	73·9, 73·4, 74·0	73·77 P.S.

Therefore 26·10 P.S. divisions = 1 millim. V.S.,

and scale reading for top of mass = $387 + \frac{75{\cdot}40 - 73{\cdot}77}{26{\cdot}10}$

= 387·062 millims. V.S.

	Reading on pointer scale.	Mean.
Lower telescope sighting 146 millims. V.S.	45·9, 45·9, 45·0	45·60 P.S.
Lower telescope sighting 145 millims. V.S.	20·4, 19·4, 20·0	19·93 P.S.

Therefore 25·77 P.S. divisions = 1 millim. V.S.,

and scale reading for bottom of mass = $145 + \frac{26{\cdot}53 - 19{\cdot}93}{25{\cdot}77}$

= 145·256 millims. V.S.

The difference = 24·1806 centims.

Subtracting the same correction as in the last case for the cross wire,

Diameter of m = 24·176 centims.

Vertical Diameters of Attracted Masses A and B taken between the Junctions of the Securing Nuts with the Sphere.

A.

	Reading on pointer scale.	Mean.
Upper telescope sighting top of mass . .	82·7, 83·0, 82·9	82·9 P.S.
Lower telescope sighting bottom of mass .	31·0, 31·5, 31·3	31·3 P.S.

TURNING round to the Vertical Scale.

	Reading on pointer scale.	Mean.
Upper telescope sighting 429 millims. V.S.	95·0, 95·2, 95·3	95·3 P.S.
Upper telescope sighting 428 millims. V.S.	69·0, 69·0, 68·9	69·0 P.S.

Therefore 26·3 P.S. divisions = 1 millim. V.S.,

and scale reading for top of mass = $428 + \frac{82{\cdot}9 - 69{\cdot}0}{26{\cdot}3}$

= 428·531 millims. V.S.

	Reading on pointer scale.	Mean.
Lower telescope sighting 271 millims. V.S.	48·0, 47·8, 48·4	48·1 P.S.
Lower telescope sighting 270 millims. V.S.	23·4, 23·0, 22·8	23·1 P.S.

Therefore 25·0 P.S. divisions = 1 millim. V.S.,

and scale reading for bottom of mass = $270 + \frac{31{\cdot}3 - 23{\cdot}1}{25{\cdot}0}$

= 270·328 millims. V.S.

The difference gives the diameter since the middle of the cross wire was used, so that

Diameter of A = 15·8203 centims.

B.

	Reading on pointer scale.	Mean.
Upper telescope sighting top of mass . .	72·0, 71·0, 71·0	71·3 P.S.
Lower telescope sighting bottom of mass .	24·6, 25·0, 25·2	24·9 P.S.

TURNING round to the Vertical Scale.

	Reading on pointer scale.	Mean.
Upper telescope sighting 430 millims. V.S.	94·0, 94·6, 94·0	94·2 P.S.
Upper telescope sighting 429 millims. V.S.	68·0, 68·1, 68·1	68·1 P.S.

Therefore 26·1 P.S. divisions = 1 millim. V.S.,

and scale reading for top of mass = $429 + \frac{71{\cdot}3 - 68{\cdot}1}{26{\cdot}1}$

= 429·123 millims. V.S.

	Reading on pointer scale.	Mean.
Lower telescope sighting 272 millims. V.S.	43·3, 43·0, 43·0	43·1 P.S.
Lower telescope sighting 271 millims. V.S.	17·1, 17·3, 17·4	17·3 P.S.

Therefore 25·8 P.S. divisions = 1 millim. V.S.,

and scale reading for bottom of mass $= 271 + \frac{24{\cdot}9 - 17{\cdot}3}{25{\cdot}8}$

$= 271{\cdot}294$ millims. V.S.

And diameter of B = 15·7829 centims.

Vertical Distances between the Levels of the Centres of the Attracting and Attracted Masses Measured by Cathetometer.

The measurements were made as soon as possible after the completion of a set of weighings, usually on the following day.

It was necessary to fix the attracted masses in the position occupied during the weighings, and with the beam of the balance in the same strained condition. This was done in some cases, by gripping the left suspending wire by a pair of jaws; in others, by adding a small weight to one side, and placing a block of the right thickness under the mass on that side.

The cathetometer was placed in front of the left side of the balance case, from which position all the masses could be viewed by turning the telescope round the central pillar (Fig. 5). It was read when sighting the top of each attracting mass and the top of each attracted mass when in the lower position, the bottom of each when in the upper position, the top and bottom being taken at the junctions of the securing nuts with the masses. It is therefore necessary to add to the distances measured by the cathetometer the difference of the radii of attracting and attracted masses in the lower position, and their sum in the upper position (see p. 119). The work is shown in full for February 5 and May 5.

Tests were made at various times, showing that there was no change in the distances (at least within errors of reading), either through moving the turntable or in the course of a few days (see February 5 and May 5 for examples).

Temperature Correction.—The cathetometer scale is taken as correct at 18°, and its coefficient of expansion is assumed to be 1/60000. That of the masses is probably about 1/40000, but, for simplicity, is taken as equal to that of the scale, the difference, 1/120000, never amounting to as much as the errors of reading, since the greatest length concerned is 23 centims.

The temperature was estimated to be about 1° above that

observed during the immediately preceding weighings, the presence of the observer and the lights used tending to raise it.

The cathetometer rested always on the brick floor of the room. Its vernier reads to ·002 centim.

Set I.

Attracted masses A on the left, B on the right. Attracting mass M moving round from left to right in front of the balance case.

February 5, 1890.—Attracted masses in upper position. Assumed temperature 11°.

Half way through the measurements the cathetometer was accidentally moved, and could not be exactly replaced. Repeating the reading of A it was found that ·197 must be added to the previous readings to compare with the following ones. This addition is made where the numbers have an asterisk.

	A. 64·999* 65·001*	B. 65·284 65·282	
m_2 23·448	M_1 25·895* 25·889*	M_2 26·070 26·064	m_1 23·947*
Differences: $A - m_2 = 41·552$	$A - M_1 = 39·108$	$B - M_2 = 39·216$	$B - m_1 = 41·336$

From Table I., p. 119, the sums of the radii of the masses are

$$R_M + R_A = 23·173,$$
$$R_M + R_B = 23·154,$$
$$R_m + R_A = 19·998,$$
$$R_m + R_B = 19·979.$$

whence $d = \frac{1}{2}\{39·108 + 23·173 + 39·216 + 23·154\}$
$= 62·326,$

and $h = \frac{1}{2}\{41·336 + 19·979 + 41·552 + 19·998\}$
$= 61·433.$

These are in terms of a scale correct at 18°, so that the value is too great by about 7/60000. We take as true values

Corrected $d = 62·318,$
$h = 61·425.$

Test Experiment.—At the conclusion, the distance $A - M_1$ was measured again and found to be 39·110.

May 28, 1890.—Attracted masses in upper position. Assumed temperature 14°.

	A.	B.	
	64·674	65·286	
	64·674	65·288	
m_2	M_1	M_2	m_1
23·422	25·726	25·920	23·766
23·424	25·724	25·920	23·756
Differences: A − m_2 = 41·552	A − M_1 = 38·949	B − M_2 = 39·367	B − m_1 = 41·526

whence $d = 62{\cdot}312,$
$h = 61{\cdot}377.$

Subtracting temperature correction ·004,

Corrected $d = 62{\cdot}308,$
$h = 61{\cdot}373.$

Mean values in Set I., $d = 62{\cdot}313,$
$h = 61{\cdot}399.$

May 5, 1890.—Attracted masses in lower position. Assumed temperature 13°.

	A.	B.	
	50·324	50·622	
	50·324	50·634	
	50·328	50·630	
m_2	M_1	M_2	m_1
23·672	25·972	26·138	23·998
23·674	25·970	26·138	24·008
	25·972	26·132	
Differences: A − m_2 = 26·652	A − M_1 = 24·354	B − M_2 = 24·493	B − m_1 = 26·626

From Table I., p. 119,

$$R_M - R_A = 7{\cdot}353,$$
$$R_M - R_B = 7{\cdot}372,$$
$$R_m - R_A = 4{\cdot}178,$$
$$R_m - R_B = 4{\cdot}197,$$

whence $D = \frac{1}{2}\{24{\cdot}354 + 7{\cdot}353 + 24{\cdot}493 + 7{\cdot}372\}$
$= 31{\cdot}786,$

and $H = \frac{1}{2}\{26{\cdot}626 + 4{\cdot}197 + 26{\cdot}652 + 4{\cdot}178\}$
$= 30{\cdot}827.$

Subtracting temperature correction ·0025.
Corrected values for Set I.,

$$D = 31{\cdot}783,$$
$$H = 30{\cdot}824.$$

Test Experiment.—The balance was set free at the end of these measures, and two days later, on May 7, it was again fixed, and the distance D was determined by the cathetometer described on p. 94. The value obtained was D = 31·786.

NOTE.—If the apparatus were perfectly rigid and constant in its dimensions, we should expect $D - H = d - h =$ constant. The values actually given by the above experiment are

February 5 . . . . . ·892,
May 5 . . . . . ·959,
May 28 . . . . . ·935.

There is apparently a slight increase during the course of the spring, probably due to the warping of the wood supporting the mass m. But there was some uncertainty in sighting the top of the mass m, especially when in the position on the right.

SET II.

Attracted masses, A on the right, B on the left. Attracting mass M moving round from left to right behind the balance case. All the masses inverted.

July 29, 1890.—Attracted masses in lower position. Assumed temperature 16°.

	B.	A.	
	49·014	49·840	
	49·014	49·844	
m_1	M_2	M_1	m_2
22·434	24·584	24·788	22·868
22·436	24·586	24·782	22·864
Differences: $B - m_1 = 26·579$	$B - M_2 = 24·429$	$A - M_1 = 25·060$	$A - m_2 = 26·979$

whence D = 32·107,
H = 30·967.

Subtracting temperature correction ·001.

Corrected D = 32·106,
H = 30·966.

September 18, 1890.—Attracted masses in lower position. Assumed temperature 16°.

	B.	A.	
	49·076	49·768	
	49·074	49·766	
m_1	M_2	M_1	m_2
22·467	24·576	24·756	22·840
	24·576	24·758	
Differences: $B - m_1 = 26·608$	$B - M_2 = 24·499$	$A - M_1 = 25·010$	$A - m_2 = 26·927$

whence $D = 82{\cdot}117$,
$H = 30{\cdot}955$.

Subtracting temperature correction ·001,

Corrected $D = 32{\cdot}116$,
$H = 30{\cdot}954$.

Mean values in Set II., $D = 32{\cdot}111$,
$H = 30{\cdot}960$.

September 27, 1890.—Attracted masses in upper position. Assumed temperature 16°.

	B.	A.	
	63·880	64·540	
	63·876	64·544	
m_1	M_2	M_1	m_2
22·450	24·570	24·756	22·810
22·448	24·572	24·758	22·816
Differences: $B - m_1 = 41{\cdot}429$	$B - M_2 = 39{\cdot}307$	$A - M_1 = 39{\cdot}785$	$A - m_2 = 41{\cdot}729$

whence $d = 62{\cdot}710$,
$h = 61{\cdot}568$.

Subtracting temperature correction ·002,

Corrected values for Set II.,

$d = 62{\cdot}708$,
$h = 61{\cdot}566$,

NOTE.—The values of D − H and and $d - h$, which should be constant, are from the above, and from another set of measures (not here recorded, see p. 115) on September 15, as follows. (We have no reason to expect the same value as in in Set I., as the masses M, m, have changed sides.)

July 29 . . . . . 1·140,
September 15 . . . . 1·140,
September 18 . . . . 1·162,
September 27 . . . . 1·142.

From July 29 to September 15 inclusive, the balance was swinging freely without alteration. The values of H should, therefore, be the same on those dates. They were

July 29 . . . . . 30·967
September 15 . . . 30·978

equal almost within errors of reading for the top of m.

Means of Sets I. and II. :

$$D = \tfrac{1}{2}(31{\cdot}783 + 32{\cdot}111) = 31{\cdot}947.$$

$$H = \tfrac{1}{2}(30{\cdot}824 + 30{\cdot}960) = 30{\cdot}892.$$

$$d = \tfrac{1}{2}(62{\cdot}313 + 62{\cdot}708) = 62{\cdot}511.$$

$$h = \tfrac{1}{2}(61{\cdot}399 + 61{\cdot}566) = 61{\cdot}483.$$

Horizontal Distances.

Set I.

$L = 123{\cdot}269$ centims.

At 18° $l_1 = 122{\cdot}915$,,

and $\frac{L}{2} = 61{\cdot}635$,,

Whence $l_1 + \frac{L}{2} = 184{\cdot}550$,,

$l_1 - \frac{L}{2} = 61{\cdot}280$,,

Taking the mean temperature of the Set as 12°, and assuming 1/60000 as the coefficient of expansion, on correcting to 12°,

$l_1 + \frac{L}{2} = 184{\cdot}532$ centims.

$l_1 - \frac{L}{2} = 61{\cdot}274$,,

Set II.

At 18° $l_2 = 122{\cdot}795$ centims.

$\frac{L}{2} = 61{\cdot}635$,,

Whence $l_2 + \frac{L}{2} = 184{\cdot}430$,,

$l_2 - \frac{L}{2} = 61{\cdot}160$,,

Taking the mean temperature of the Set as 15°, and correcting to 15°,

$$l_2 + \frac{L}{2} = 184{\cdot}421 \text{ centims.}$$

$$l_2 - \frac{L}{2} = 61{\cdot}157 \quad ,,$$

Mean values for the two Sets

$$L = 123{\cdot}260 \quad ,,$$

$$l + \frac{L}{2} = 184{\cdot}477 \quad ,,$$

$$l - \frac{L}{2} = 61{\cdot}216 \quad ,,$$

TABLE III.—Determination of Attraction by the Balance.

Determinations of Attraction in Terms of the Riders by the Balance.

In each case, four turning-points of three successive swings are recorded in tenths of a division—*i.e.*, in divisions on the diagonal lines. In the columns headed *i* the masses and riders are in the initial position, in those headed *r* the riders are moved, and in those headed *m* the masses are moved. Under each set of four readings is the calculated centre of swing (p. 101). In the next line are the deflections due to movements of riders and masses, each placed under the middle one of the three centres of swing from which it is calculated. In the next line are the values of deflection due to mass ÷ deflection due to rider, or M/R (p. 103).

SET I.

I. ATTRACTED Masses in Upper Position. Feb. 4, 1890, 7.59 P.M. to 10·49 P.M. Temperature: in observing room, 15°·7–16°·5; in balance room, 10°·05. Barometer 752·2–752·0 millims. Weather mild and still, after slight frost on the two previous nights. Time between successive passages of centre about 20 seconds.

	i. (1)	*r.* (2)	*i.* (3)	*m.* (4)	*i.* (5)	*r.* (6)	*i.* (7)	*m.* (8)
Scale readings . .	725	912	725	804	764	913	726	804
	798	838	799	787	779	838	800	787
	759	878	759	796	771	879	759	797
	780	856	781	791	776	857	781	791
Centre of swing . .	772·55	863·85	773·00	792·80	773·90	864·60	773·40	793·20
Deflection due to rider or mass . .	—	91·075	—	19·350	—	90·950	—	19·700
Mass deflection ÷ rider deflection .	—	—	—	·212608	—	·214688	—	·217110

TABLE III.—(*continued*).

	i. (9)	*r.* (10)	*i.* (11)	*m.* (12)	*i.* (13)	*r.* (14)	*i.* (15)	*m.* (16)
Scale readings . .	763	913	724	804	764	914	725	805
	779	837	801	787	779	838	801	789
	771	879	759	796	771	880	760	796
	775	857	782	792	776	857	783	792
Centre of swing . .	773·60	864·25	773·85	793·05	773·90	865·05	774·50	793·65
Deflection due to rider or mass . .	—	90·525	—	19·175	—	90·850	—	18·950
Mass deflection ÷ rider deflection .	—	·214720	—	·211440	—	·209824	—	·208758

	i. (17)	*r.* (18)	*i.* (19)	*m.* (20)	*i.* (21)	*r.* (22)	*i.* (23)	*m.* (24)
Scale readings. . .	765	916	726	803	765	914	725	805
	780	839	803	788	779	838	800	787
	772	881	762	797	771	879	759	796
	777	859	784	792	776	857	782	791
Centre of swing . .	774·90	866·30	776·30	793·65	774·00	864·50	773·60	792·85
Deflection due to rider or mass . .	—	90·700	—	18·500	—	90·700	—	19·225
Mass deflection ÷ rider deflection .	—	·206174	—	·203966	—	·207966	—	·211438

	i. (25)	*r.* (26)	*i.* (27)	*m.* (28)	*i.* (29)	*r.* (30)	*i.* (31)	*m.* (32)
Scale readings . .	763	914	727	805	764	913	725	804
	780	838	800	789	779	838	800	786
	770	880	760	797	771	879	760	796
	776	857	782	792	775	857	781	791
Centre of swing . .	773·65	865·05	774·15	793·90	773·65	864·65	773·80	792·50
Deflection due to rider or mass . .	—	91·150	—	20·000	—	90·925	—	19·275
Mass deflection ÷ rider deflection. .	—	·215167	—	·219690	—	·215975	—	·211494

	i. (33)	*r.* (34)	*i.* (35)	*m.* (36)	*i.* (37)	*r.* (38)	*i.* (39)	*m.* (40)
Scale readings . .	763	913	724	803	763	912	726	802
	778	838	801	789	778	837	799	787
	770	879	759	796	769	879	759	797
	774	857	782	792	774	857	780	792
Centre of Swing. .	772·65	864·60	773·85	793·50	772·30	864·20	772·95	793·30
Deflection due to rider or mass . .	—	91·350	—	20·425	—	91·575	—	19·875
Mass deflection ÷ rider deflection. .	—	·217296	—	·223316	—	·220038	—	·216503

TABLE III.—(*continued*).

	i. (41)	*r.* (42)	*i.* (43)	*m.* (44)	*i.* (45)	*r.* (46)	*i.* (47)	*m.* (48)
Scale readings . .	764	915	725	803	763	913	726	803
	779	839	800	786	778	838	799	787
	771	880	759	795	770	879	759	796
	776	858	780	791	774	857	781	792
Centre of swing . .	773·90	865·55	773·15	792·25	772·70	864·60	773·15	792·95
Deflection due to rider or mass . .	—	92·025	—	19·325	—	91·675	—	19·200
Mass deflection ÷ rider deflection .	—	·212986	—	·210397	—	·210117	—	·209693

	i. (49)	*r.* (50)	*i.* (51)	*m.* (52)	*i.* (53)	*r.* (54)	*i.* (55)	*m.* (56)
Scale readings . .	764	914	724	803	762	912	721	801
	779	839	802	787	778	836	798	785
	772	880	759	795	769	877	755	794
	776	858	781	799	774	855	779	788
Centre of swing . .	774·35	865·55	773·85	792·10	772·20	862·55	770·35	790·55
Deflection due to rider or mass . .	—	91·450	—	19·075	—	91·275	—	20·275
Mass deflection ÷ rider deflection .	—	·209267	—	·208784	—	·215557	—	·222161

	i. (57)	*r.* (58)	*i.* (59)	*m.* (60)	*i.* (61)	*r.* (62)	*i.* (63)*	*i.* (63*a*)	*m.* (64)
Scale readings	760	911	724	803	762	911	722	725	803
	776	836	799	785	777	836	800	799	786
	767	877	758	795	769	877	758	759	796
	772	855	779	789	773	855	780	780	790
Centre of swing	770·20	862·50	772·20	791·35	771·70	862·50	772·50	772·90	792·30
Deflection due to rider or mass . . .	—	91·250	—	19·425	—	90·400	—	—	19·900
Mass deflection ÷ rider deflection .	—	·217534	—	·213873	—	·217506	—	—	·217873

* After 63 the riders were moved by mistake instead of the masses, therefore it was necessary to return to the initial position, and take the readings in (63*a*).

TABLE III.—(*continued*).

	i. (65)	*r.* (6	*i.* (67)	*m.* (68)	*i.* (69)	*r.* (70)	*i.* (71)	*m.* (72)
Scale readings . .	762	913	725	802	759	909	722	802
	777	838	800	785	776	834	797	784
	769	879	758	794	767	875	757	794
	774	857	780	789	772	854	779	789
Centre of swing . .	771·90	864·60	772·75	790·80	770·15	860·80	771·05	790·55
Deflection due to rider or mass . .	—	92·275	—	19·350	—	90·200	—	19·450
Mass deflection ÷ rider deflection .	—	·213170	—	·212084	—	·215078	—	·214947

	i. (73)	*r.* (74)	*i.* (75)	*m.* (76)	*i.* (77)	*r.* (78)	*i.* (79)	*m.* (80)
Scale readings . .	760	911	724	803	762	911	721	800
	777	835	798	785	777	835	797	784
	768	877	757	795	769	877	756	793
	773	854	779	790	773	854	778	788
Centre of swing . .	771·15	862·05	771·40	791·50	771·70	862·05	770·30	789·75
Deflection due to rider or mass . .	—	90·775	—	19·950	—	91·050	—	20·150
Mass deflection ÷ rider deflection .	—	·217020	—	·219442	—	·220209	—	·221064

	i. (81)	*r.* (82)	*i.* (83)	*m.* (84)	*i.* (85)	*r.* (86)	*i.* (87)	*m.* (88)
Scale readings . .	759	910	722	801	759	910	723	802
	774	833	796	783	775	834	797	783
	766	876	757	793	767	876	757	793
	771	854	778	787	771	854	778	787
Centre of swing . .	768·90	860·95	770·50	789·30	769·60	861·25	770·90	789·35
Deflection due to rider or mass . .	—	91·250	—	19·250	—	91·000	—	19·300
Mass deflection ÷ rider deflection .	—	·215990	—	·211248	—	·211813	—	·212995

	i. (89)	*r.* (90)	*i.* (91)	*m.* (92)	*i.* (93)	*r.* (94)	*i.* (95)	*m.* (96)
Scale readings . .	759	908	719	800	760	910	723	800
	775	831	795	783	775	835	798	785
	766	874	754	792	767	876	756	793
	771	852	776	788	772	854	779	789
Centre of swing . .	769·20	859·00	768·35	789·05	769 90	861·60	770·90	790·30
Deflection due to rider or mass . .	—	90·225	—	19·925	—	91·200	—	19·350
Mass deflection ÷ rider deflection .	—	·217373	—	·219650	—	·215323	—	·212462

TABLE III.—(*continued*).

	i. (97)	*r.* (98)	*i.* (99)	*m.* (100)	*i.* (101)	*r.* (102)	*i.* (103)	*m.* (104)	*i.* (105)
Scale readings	761	910	721	798	759	909	721	800	759
	777	835	796	783	775	833	796	783	775
	768	876	756	791	765	874	756	793	767
	772	853	777	787	770	852	777	787	771
Centre of swing	771·00	861·35	769·80	788·35	768·60	859·65	769·80	789·35	769·60
Deflection due to rider or mass . . .	—	90·950	—	19·150	—	90·450	—	19·650	—
Mass deflection ÷ rider deflection .	—	·211655	—	·211136	—	·214483	—	—	—

Feb. 4, 1890. Mean of 50 determinations of $M/R = a$ } ·21422122.
Attracted masses in upper position }

II. ATTRACTED Masses in Lower Position. April 30, 1890, 7.45 P.M. to 10.32 P.M. Temperature: in observing room, 17°–16°·1; in balance room, 11°·1; barometer, 748·6–749·2 millims. Weather clear; S.E. wind; sunny during day. Time between successive passages of centre not quite 20 seconds.

	i. (1)	*r.* (2)	*i.* (3)	*m.* (4)	*i.* (5)	*r.* (6)	*i.* (7)	*m.* (8)
Scale readings . .	1046	1133	951	1127	955	1134	952	1123
	969	1055	1024	1062	1025	1059	1028	1069
	1012	1098	984	1099	986	1102	985	1099
	988	1075	1007	1078	1007	1077	1009	1082
Centre of swing . .	996·60	1082·85	998·35	1085·50	999·80	1086·25	1000·40	1088·25
Deflection due to rider or mass .	—	85·375	—	86·425	—	86·150	—	87·350
Mass deflection ÷ rider deflection .	—	—	—	1·00772	—	1·00856	—	1·01437

TABLE III.—(*continued*).

	i. (9)	*r.* (10)	*i.* (11)	*m.* (12)	*i.* (13)	*r.* (14)	*i.* (15)	*m.* (16)
Scale readings . .	962 1023 989 1009	1136 1060 1104 1079	955 1029 987 1012	1129 1067 1102 1083	961 1027 989 1012	1136 1064 1105 1081	956 1031 989 1013	1133 1069 1103 1085
Centre of swing . .	1001·40	1088·00	1002·45	1089·55	1003·15	1090·00	1004·15	1091·25
Deflection due to rider or mass .	—	86·075	—	86·750	—	86·350	—	86·350
Mass deflection ÷ rider deflection .	—	1·01133	—	1·00623	—	1·00232	—	1·00101

	i. (17)	*r.* (18)	*i.* (19)	*m.* (20)	*i.* (21)	*r.* (22)	*i.* (23)	*m.* (24)
Scale readings . .	967 1027 994 1012	1141 1064 1108 1083	957 1034 990 1015	1135 1070 1106 1086	965 1031 994 1015	1143 1066 1110 1085	958 1036 993 1017	1134 1073 1108 1089
Centre of swing . .	1005·65	1092·00	1006·00	1093·20	1007·40	1094·00	1008·35	1095·40
Deflection due to rider or mass .	—	86·175	—	86·500	—	86·125	—	86·825
Mass deflection ÷ rider deflection .	—	1·00290	—	1·00406	—	1·00624	—	1·01106

	i. (25)	*r.* (26)	*i.* (27)	*m.* (28)	*i.* (29)	*r.* (30)	*i.* (31)	*m.* (32)
Scale readings . .	976 1027 998 1016	1143 1069 1110 1087	963 1037 996 1019	1141 1073 1112 1090	966 1037 996 1019	1145 1070 1112 1089	960 1040 995 1020	1141 1075 1113 1092
Centre of swing . .	1008·80	1095·35	1010·65	1097·90	1010·85	1097·05	1011·15	1099·30
Deflection due to rider or mass .	—	85·625	—	87·150	—	86·050	—	87·575
Mass deflection ÷ rider deflection .	—	1·01591	—	1·01529	—	1·01264	—	1·01713

TABLE III.—(*continued*).

	i. (33)	*r.* (34)	*i.* (35)	*m.* (36)	*i.* (37)	*r.* (38)	*i.* (39)	*m.* (40)
Scale readings . .	973 1034 1000 1020	1147 1071 1114 1090	962 1041 996 1022	1137 1079 1112 1094	975 1034 1002 1020	1146 1075 1114 1091	965 1042 999 1021	1138 1080 1113 1094
Centre of swing . .	1012·30	1098·55	1012·50	1100·20	1013·40	1099·80	1014·00	1101·00
Deflection due to rider or mass. .	—	86·150	—	87·250	—	86·100	—	86·650
Mass deflection ÷ rider deflection.	—	1·01465	—	1·01306	—	1·00987	—	1·00858

	i. (41)	*r.* (42)	*i.* (43)	*m.* (44)	*i.* (45)	*r.* (46)	*i.* (47)	*m.* (48)
Scale readings . .	977 1035 1003 1022	1150 1073 1116 1093	964 1043 1000 1025	1089 1110 1098 1104	976 1038 1004 1023	1153 1074 1118 1094	968 1044 1001 1025	1148 1078 1118 1095
Centre of swing . .	1014·70	1100·80	1015·45	1102·20	1016·15	1102·35	1016·45	1103·40
Deflection due to rider or mass. .	—	85·725	—	86·400	—	86·050	—	86·475
Mass deflection ÷ rider deflection.	—	1·00933	—	1·00597	—	1·00450	—	1·00625

	i. (49)	*r.* (50)	*i.* (51)	*m.* (52)	*i.* (53)	*r.* (54)	*i.* (55)	*m.* (56)
Scale readings . .	978 1039 1005 1025	1153 1076 1119 1094	969 1045 1002 1027	1149 1080 1118 1097	976 1043 1005 1026	1153 1079 1121 1096	968 1047 1004 1028	1145 1085 1118 1099
Centre of swing . .	1017·40	1103·35	1017·65	1104·45	1018·60	1105·55	1019·25	1106·15
Deflection due to rider or mass .	—	85·825	—	86·325	—	86·625	— .	86·875
Mass deflection ÷ rider deflection.	—	1·00670	—	1·00116	—	·99971	—	1·00973

TABLE III.—(*continued*).

	i. (57)	*r.* (58)	*i.* (59)	*m.* (60)	*i.* (61)	*r.* (62)	*i.* (63)	*m.* (64)
Scale readings . .	984 1039 1008 1026	1155 1078 1120 1097	971 1048 1004 1029	1151 1083 1122 1100	977 1046 1007 1030	1157 1081 1123 1100	972 1051 1007 1031	1152 1087 1123 1102
Centre of swing . .	1019·30	1105·10	1020·00	1107·85	1021·25	1108·10	1022·60	1109·95
Deflection due to rider or mass .	—	85·450	—	87·225	—	86·175	—	86·850
Mass deflection ÷ rider deflection .	—	1·01872	—	1·01646	—	1·01011	—	1·00798

	i. (65)	*r.* (66)	*i.* (67)	*m.* (68)	*i.* (69)	*r.* (70)	*i.* (71)	*m.* (72)
Scale readings . .	983 1046 1011 1031	1159 1082 1125 1102	976 1051 1008 1033	1153 1088 1126 1104	983 1049 1012 1032	1161 1083 1127 1102	978 1053 1011 1034	1158 1088 1127 1106
Centre of swing . .	1023·60	1109·80	1023·70	1112·05	1025·15	1111·05	1025·95	1113·15
Deflection due to rider or mass .	—	86·150	—	87·625	—	85·500	—	86·700
Mass deflection ÷ rider deflection .	—	1·01262	—	1·02097	—	1·01944	—	1·01226

	i. (73)	*r.* (74)	*i.* (75)	*m.* (76)	*i.* (77)	*r.* (78)	*i.* (79)	*m.* (80)
Scale readings . .	985 1051 1014 1033	1163 1086 1129 1104	980 1056 1013 1036	1156 1092 1129 1108	990 1051 1017 1036	1163 1087 1131 1107	982 1057 1015 1039	1161 1093 1132 1109
Centre of swing . .	1026·95	1113·40	1028·25	1115·50	1029·20	1115·20	1030·15	1117·65
Deflection due to rider or mass .	—	85·880	—	86·775	—	85·525	—	87·100
Mass deflection ÷ rider deflection .	—	1·01093	—	1·01299	—	1·01652	—	1·01782

TABLE III.—(*continued*).

	i. (81)	*r.* (82)	*i.* (83)	*m.* (84)	*i.* (85)	*r.* (86)	*i.* (87)	*m.* (88)
Scale readings . .	991	1167	984	1158	992	1169	984	1161
	1054	1090	1059	1098	1056	1092	1063	1100
	1018	1133	1018	1132	1021	1136	1018	1135
	1038	1108	1041	1112	1041	1111	1042	1115
Centre of swing . .	1030·95	1117·40	1032·60	1119·55	1033·55	1120·00	1034·00	1122·20
Deflection due to rider or mass .	—	85·625	—	86·475	—	86·225	—	87·600
Mass deflection ÷ rider deflection.	—	1·01358	—	1·00640	—	1·00942	—	1·01890

	i. (89)	*r.* (90)	*i.* (91)	*m.* (92)	*i.* (93)	*r.* (94)	*i.* (95)	*m.* (96)
Scale readings . .	996	1171	987	1165	995	1172	989	1169
	1058	1094	1064	1100	1061	1097	1066	1099
	1022	1137	1022	1137	1024	1137	1023	1140
	1043	1114	1045	1117	1045	1116	1046	1117
Centre of swing . .	1035·20	1121·75	1036·85	1123·75	1037·35	1123·15	1038·20	1125·05
Deflection due to rider or mass .	—	85·725	—	86·650	—	85·375	—	86·575
Mass deflection ÷ rider deflection.	—	1·01633	—	1·01286	—	1·01450	—	1·01065

	i. (97)	*r.* (98)	*i.* (99)	*m* (100)	*i.* (101)	*r.* (102)	*i.* (103)	*m.* (104)	*i.* (105)
Scale readings . .	998	1175	992	1174	995	1176	995	1169	1001
	1062	1097	1066	1102	1064	1098	1067	1105	1066
	1026	1141	1025	1141	1027	1143	1026	1143	1029
	1045	1116	1047	1119	1048	1118	1049	1121	1049
Centre of swing . .	1038·75	1125·05	1039·45	1127·15	1040·15	1126·70	1040·75	1128·90	1042·20
Deflection due to rider or mass .	—	85·950	—	87·350	—	86·250	—	87·425	
Mass deflection ÷ rider deflection.	—	1·01178	—	1·01452	—	1·01319	—	—	

April 30. Mean of 50 determinations of M/R = A } 1·010905.
Attracted masses in lower position }

TABLE III.—(*continued*).

MAY 4, 1890, 11.11 to 11.50 A.M. Temperature: in observing room, 13°·5–13°·8; in balance room, 11°·7; barometer, 742·0–741·7 millims. Weather inclined to rain; a little cooler than previous day; wind S. to S.W.

	i. (1)	*r.* (2)	*i.* (3)	*m.* (4)	*i.* (5)	*r.* (6)	*i.* (7)	*m.* (8)
Scale readings . .	875	1045	865	1044	865	1045	861	1041
	936	969	939	970	938	967	940	971
	900	1013	896	1014	897	1013	894	1012
	920	988	921	989	920	986	921	989
Centre of swing . .	913·10	996·95	911·80	997·75	911·60	996·00	910·95	997·10
Deflection due to rider or mass . .	—	84·50	—	86·050	—	84·725	—	86·275
Mass deflection ÷ rider deflection .	—	—	—	1·01699	—	1·01697	—	1·01950

	i. (9)	*r.* (10)	*i.* (11)	*m.* (12)	*i.* (13)	*r.* (14)	*i.* (15)	*m.* (16)
Scale readings .	869	1046	862	1036	867	1045	860	1040
	936	966	939	972	936	964	937	968
	896	1012	894	1009	896	1011	893	1009
	919	985	920	988	918	984	919	986
Centre of swing .	910·700	995·10	910·45	995·55	910·40	993·80	909·20	994·20
Deflection due to rider or mass. .	—	84·525	—	85·125	—	84·000	—	85·450
Mass deflection ÷ rider deflection .	—	1·01390	—	1·01024	—	1·01533	—	1·01454

	i. (17)	*r.* (18)	*i.* (19)	*m.* (20)	*i.* (21)	*r.* (22)	*i.* (23)	*m.* (24)	*i.* (25)
Scale readings .	863	1043	859	1033	863	1040	857	1035	862
	934	964	936	971	932	963	936	966	930
	894	1009	892	1006	892	1007	889	1006	891
	916	983	917	985	915	982	915	983	914
Centre of swing	908·30	992·60	908·00	993·15	906·60	991·00	906·10	991·40	905·25
Deflection due to rider or mass .	—	84·450	—	85·850	—	84·650	—	85·725	
Mass deflection ÷ rider deflection	—	1·01421	—	1·01538	—	1·01344	—	—	

May 4. Morning. Mean of 10 determinations of M/R = A } 1·015050.
Attracted masses in lower position }

TABLE III.—(*continued*).

MAY 4, 1890.—*Same day.* 2.40 to 4.54 P.M. Temperature: in observing room, 13°·9–14°·1; in balance room, 11°·7–11°·75; barometer, 740·3–739·7.

	i. (1)	*r.* (2)	*i.* (3)	*m.* (4)	*i.* (5)	*r.* (6)	*i.* (7)	*m.* (8)
Scale readings . .	847	1035	853	1031	864	1035	853	1026
	933	957	930	961	925	960	931	965
	883	1003	886	1002	890	1003	885	1001
	912	977	911	979	909	977	912	980
Centre of swing . .	901·40	986·20	902·00	987·05	902·55	987·10	902·00	987·65
Deflection due to rider or mass . .	—	84·500	—	84·775	—	84·825	—	85·300
Mass deflection ÷ rider deflection .	—	—	—	1·00133	—	1·00251	—	1·00783

	i. (9)	*r.* (10)	*i.* (11)	*m.* (12)	*i.* (13)	*r.* (14)	*i.* (15)	*m.* (16)
Scale readings . .	866	1035	853	1023	864	1036	854	1024
	924	959	932	968	925	960	931	968
	891	1004	886	1000	889	1004	886	1000
	909	977	912	982	910	978	912	982
Centre of swing . .	902·70	987·20	902·80	988·35	902·30	987·75	902·50	988·45
Deflection due to rider or mass . .	—	84·450	—	85·800	—	85·350	—	85·925
Mass deflection ÷ rider deflection .	—	1·01303	—	1·01060	—	1·00601	—	1·00940

	i. (17)	*r.* (18)	*i.* (19)	*m.* (20)	*i.* (21)	*r.* (22)	*i.* (23)	*m.* (24)
Scale readings . .	864	1039	855	1025	866	1040	855	1011
	925	958	931	969	925	958	932	977
	890	1005	887	1001	891	1005	888	996
	909	978	913	982	911	978	913	985
Centre of swing . .	902·55	987·80	903·25	989·20	903·50	987·90	904·00	989·10
Deflection due to rider or mass . .	—	84·900	—	85·825	—	84·150	—	85·225
Mass deflection ÷ rider deflection .	—	1·01148	—	1·01538	—	1·01634	—	1·01232

TABLE III.—(*continued*).

	i. (25)	*r.* (26)	*i.* (27)	*m.* (28)	*i.* (29)	*r.* (30)	*i.* (31)	*m.* (32)
Scale readings . .	864	1036	854	1024	865	1037	854	1031
	927	961	933	970	926	962	934	967
	890	1004	887	1001	892	1005	888	1004
	912	979	914	984	912	980	915	983
Centre of swing . .	903·75	988·10	904·00	989·85	904·35	989·25	904·90	990·55
Deflection due to rider or mass . .	—	84·225	—	85·675	—	84·625	—	85·625
Mass deflection ÷ rider deflection .	—	1·01454	—	1·01481	—	1·01211	—	1·01182

	i. (33)	*r.* (34)	*i.* (35)	*m.* (36)	*i.* (37)	*r.* (38)	*i.* (39)	*m.* (40)
Scale readings . .	864	1039	855	1024	864	1041	856	1025
	928	961	934	972	929	961	934	971
	892	1006	888	1002	891	1006	888	1002
	912	980	914	985	913	980	915	984
Centre of swing . .	904·95	989·50	904·80	991·10	905·00	989·65	905·00	990·65
Deflection due to rider or mass . .	—	84·625	—	86·200	—	84·650	—	85·500
Mass deflection ÷ rider deflection .	—	1·01521	—	1·01846	—	1·01418	—	1·00796

	i. (41)	*r.* (42)	*i.* (43)	*m.* (44)	*i.* (45)	*r.* (46)	*i.* (47)	*m.* (48)
Scale readings . .	866	1038	857	1030	865	1043	858	1035
	927	962	934	960	930	962	934	966
	893	1007	889	1004	893	1008	891	1006
	913	981	915	984	915	981	915	984
Centre of swing . .	905·30	990·40	905·50	991·30	906·60	991·15	906·55	991·55
Deflection due to rider or mass . .	—	85·000	—	85·250	—	84·575	—	84·95
Mass deflection ÷ rider deflection .	—	1·00441	—	1·00546	—	1·00621	—	1·00741

	i. (49)	*r.* (50)	*i.* (51)	*m.* (52)	*i.* (53)	*r.* (54)	*i.* (55)	*m.* (56)
Scale readings . .	869	1041	858	1037	867	1041	856	1029
	927	961	935	965	929	963	936	972
	895	1008	891	1007	894	1007	890	1004
	914	982	916	984	914	982	916	986
Centre of swing . .	906·65	990·90	907·00	991·80	906·60	991·05	906·70	992·50
Deflection due to rider or mass . .	—	84·075	—	85·000	—	84·440	—	85·750
Mass deflection ÷ rider deflection .	—	1·01070	—	1·00905	—	1·01155	—	1·01509

TABLE III.—(*continued*).

	i. (57)	*r.* (58)	*i.* (59)	*m.* (60)	*i.* (61)	*r.* (62)	*i.* (63)	*m.* (64)
Scale readings . .	871	1041	859	1035	869	1044	860	1030
	928	963	936	969	931	963	937	973
	895	1008	890	1008	895	1010	892	1005
	913	982	917	985	916	983	917	986
Centre of swing . .	906·80	991·50	907·10	993·50	908·20	992·85	908·25	993·30
Deflection due to rider or mass . .	—	84·550	—	85·850	—	84·625	—	84·775
Mass deflection ÷ rider deflection .	—	1·01478	—	1·01493	—	1·00812	—	1·00162

	i. (65)	*r.* (66)	*i.* (67)	*m.* (68)	*i.* (69)	*r.* (70)	*i.* (71)	*m.* (72)
Scale readings . .	863	1042	863	1039	840	1042	861	1037
	935	965	935	969	949	965	937	971
	894	1010	894	1008	886	1009	893	1009
	917	984	917	985	923	985	918	987
Centre of swing . .	908·80	993·45	908·80	993·75	909·15	993·25	909·05	995·05
Deflection due to rider or mass . .	—	84·650	—	84·775	—	84·150	—	85·750
Mass deflection ÷ rider deflection .	—	1·00148	—	1·00444	—	1·01322	—	1·01449

	i. (73)	*r.* (74)	*i.* (75)	*m.* (76)	*i.* (77)	*r.* (78)	*i.* (79)	*m.* (80)
Scale readings . .	865	1045	860	1038	868	1045	863	1041
	935	965	938	969	934	965	937	969
	895	1011	893	1010	895	1012	894	1011
	918	985	919	987	918	985	919	988
Centre of swing . .	909·55	994·30	909·25	995·00	909·50	994·75	909·80	995·80
Deflection due to rider or mass . .	—	84·900	—	85·625	—	85·100	—	85·625
Mass deflection ÷ rider deflection .	—	1·00928	—	1·00735	—	1·00617	—	1·00765

	i. (81)	*r.* (82)	*i.* (83)	*m.* (84)	*i.* (85)
Scale readings	864	1044	860	1036	867
	938	967	940	974	936
	895	1012	894	1010	896
	919	986	920	989	919
Centre of swing	910·55	995·45	910·65	996·80	910·60
Deflection due to rider or mass . . .	—	84·850	—	86·175	
Mass deflection ÷ rider deflection . .	—	1·01238	—	—	

TABLE III.—(*continued*).

May 4, afternoon.	Mean of 40 determinations of M/R = A Attracted masses in lower position	1·0100278.
April 30 and May 4.	Mean of 100 determinations of M/R = A Attracted masses in lower position	1·0109685.

III.—ATTRACTED Masses in Upper Position. May 25, 1890; 11.20 to 12.53 noon. Temperature: in observing room, 15°·4–16°; in balance room, 13°·3; barometer, 748·5–748·1 millims. Weather, E. wind, warm, very bright. Time of swing not recorded.

	i. (1)	*r.* (2)	*i.* (3)	*m.* (4)	*i.* (5)	*r.* (6)	*i.* (7)	*m.* (8)
Scale readings . .	1071 986 1033 1005	1175 1085 1134 1108	960 1047 998 1025	1049 1028 1041 1034	1003 1021 1010 1017	1173 1085 1134 1107	956 1047 996 1024	1049 1028 1040 1034
Centre of swing . .	1015·90	1116·90	1015·50	1036·20	1014·25	1116·55	1014·25	1035·80
Deflection due to rider or mass .	—	101·200	—	21·325	—	102·300	—	21·500
Mass deflection ÷ rider deflection.	—	—	—	·209582	—	·209311	—	·210320

	i. (9)	*r.* (10)	*i.* (11)	*m.* (12)	*i.* (13)	*r.* (14)	*i.* (15)	*m.* (16)
Scale readings . .	1001 1021 1011 1016	1173 1085 1134 1106	958 1046 996 1024	1049 1028 1038 1033	1002 1020 1010 1015	1173 1084 1134 1105	957 1046 995 1023	1049 1028 1040 1032
Centre of swing . .	1014·35	1116·35	1014·05	1034·70	1013·50	1115·80	1013·25	1035·90
Deflection due to rider or mass .	—	102·150	—	20·925	—	102·425	—	22·850
Mass deflection ÷ rider deflection .	—	·207660	—	·204571	—	·213688	—	·223297

TABLE III.—(*continued*).

	i. (17)	*r.* (18)	*i.* (19)	*m.* (20)	*i.* (21)	*r.* (22)	*i.* (23)	*m.* (24)
Scale readings. . .	1003 1019 1009 1015	1173 1082 1133 1104	956 1043 994 1023	1048 1025 1038 1032	1001 1019 1008 1014	1172 1081 1131 1103	958 1042 994 1021	1048 1026 1038 1030
Centre of swing . .	1012·85	1114·60	1011·90	1033·60	1012·05	1113·10	1011·35	1033·50
Deflection due to rider or mass .	—	102·225	—	21·625	—	101·400	—	22·375
Mass deflection ÷ rider deflection.	—	·217535	—	·212400	—	·216962	—	·220172

	i. (25)	*r.* (26)	*i.* (27)	*m.* (28)	*i.* (29)	*r.* (30)	*i.* (31)	*m.* (32)
Scale readings. . .	1000 1017 1007 1014	1171 1081 1130 1103	953 1044 992 1021	1047 1025 1037 1031	1000 1016 1007 1013	1171 1080 1130 1103	955 1041 992 1021	1046 1025 1036 1030
Centre of swing . .	1010·90	1112·70	1019·80	1032·90	1010·45	1112·40	1010·00	1032·15
Deflection due to rider or mass .	—	101·850	—	22·275	—	102·175	—	22·050
Mass deflection ÷ rider deflection.	—	·219195	—	·218356	—	·216907	—	·215885

	i. (33)	*r.* (34)	*i.* (35)	*m.* (36)	*i.* (37)	*r.* (38)	*i.* (39)	*m.* (40)
Scale readings . .	997 1017 1006 1013	1168 1080 1131 1102	952 1043 992 1021	1046 1024 1037 1030	999 1018 1007 1012	1170 1082 1130 1102	955 1043 993 1021	1046 1026 1039 1030
Centre of swing . .	1010·20	1112·40	1010·40	1032·35	1010·70	1112·65	1011·05	1033·75
Deflection due to rider or mass .	—	102·100	—	21·800	—	101·775	—	22·100
Mass deflection ÷ rider deflection.	—	·214740	—	·213857	—	·215672	—	·216858

TABLE III.—(*continued*).

	i. (41)	*r.* (42)	*i.* (43)	*m.* (44)	*i.* (45)	*r.* (46)	*i.* (47)	*m.* (48)
Scale readings. . .	998 1019 1009 1014	1171 1082 1132 1104	955 1043 994 1022	1046 1027 1038 1031	1000 1019 1009 1014	1173 1082 1131 1104	956 1046 995 1023	1048 1028 1039 1032
Centre of swing . .	1012·25	1114·00	1011·65	1033·85	1012·35	1113·80	1013·20	1034·85
Deflection due to rider or mass .	—	102·050	—	21·850	—	101·025	—	21·900
Mass deflection ÷ rider deflection.	—	·215388	—	·215197	—	·216531	—	·216350

	i. (49)	*r.* (50)	*i.* (51)	*m.* (52)	*i.* (53)	*r.* (54)	*i.* (55)
Scale readings . .	1001 1019 1009 1015	1172 1083 1132 1105	956 1046 996 1023	1048 1027 1039 1032	1000 1020 1009 1016	1173 1082 1133 1105	956 1044 995 1023
Centre of swing . .	1012·70	1114·60	1013·65	1034·60	1013·15	1114·70	1012·65
Deflection due to rider or mass . .	—	101·425	—	21·200	—	101·800	—
Mass deflection ÷ rider deflection. .	—	·212472	—	·208636	—	—	—

May 25. Morning. Mean of 25 determinations of $M/R = a$ } ·21446168.
Attracted masses in upper position }

Same day. 3.15 to 4.50 P.M. Temperature: in observing room, 16°·0–16°·25; in balance room, 13°·3–13°·35; barometer, 747·7–747·4 millims.

	i. (1)	*r.* (2)	*i.* (3)	*m.* (4)	*i.* (5)	*r.* (6)	*i.* (7)	*m.* (8)
Scale readings. . .	1001 1069 1031 1052	1205 1116 1165 1138	990 1077 1029 1057	1081 1061 1073 1066	1034 1055 1044 1049	1207 1120 1168 1139	991 1080 1030 1059	1083 1062 1076 1068
Centre of swing . .	1044·55	1147·60	1046·30	1068·55	1047·60	1150·50	1048·35	1070·65
Deflection due to rider or mass .	—	102·175	—	21·600	—	102·525	—	21·675
Mass deflection ÷ rider deflection.	—	—	—	·211041	—	·211046	—	·212162

TABLE III.—(*continued*).

	i. (9)	*r.* (10)	*i.* (11)	*m.* (12)	*i.* (13)	*r.* (14)	*i.* (15)	*m.* (16)
Scale readings. . .	1037	1209	995	1086	1039	1213	994	1088
	1056	1119	1082	1066	1058	1121	1086	1069
	1046	1169	1030	1078	1048	1172	1034	1078
	1052	1141	1059	1071	1054	1145	1064	1073
Centre of swing . .	1049·60	1151·10	1049·00	1073·50	1051·60	1154·10	1052·90	1074·90
Deflection due to rider or mass .	—	101·800	—	23·200	—	101·850	—	21·675
Mass deflection ÷ rider deflection.	—	·220408	—	·227842	—	·220299	—	·216738

	i. (17)	*r.* (18)	*i.* (19)	*m.* (20)	*i.* (21)	*r.* (22)	*i.* (23)	*m.* (24)
Scale readings. . .	1045	1212	996	1088	1043	1215	1002	1094
	1059	1126	1088	1071	1063	1126	1088	1072
	1050	1175	1037	1080	1053	1177	1039	1083
	1056	1147	1066	1076	1058	1148	1066	1077
Centre of swing . .	1053·55	1157·20	1055·30	1077·10	1056·30	1158·50	1056·55	1079·25
Deflection due to rider or mass .	—	102·775	—	21·300	—	102·075	—	22·350
Mass deflection ÷ rider deflection.	—	·209073	—	·207957	—	·213813	—	·219575

	i. (25)	*r.* (26)	*i.* (27)	*m.* (28)	*i.* (29)	*r.* (30)	*i.* (31)	*m.* (32)
Scale readings. . .	1044	1217	999	1095	1048	1221	1002	1096
	1064	1127	1093	1073	1067	1131	1094	1076
	1053	1178	1041	1086	1056	1180	1042	1088
	1061	1150	1069	1079	1061	1152	1071	1081
Centre of swing . .	1057·25	1159·80	1059·35	1081·35	1059·70	1162·50	1060·70	1083·55
Deflection due to rider or mass. .	—	101·500	—	21·825	—	102·300	—	22·800
Mass deflection ÷ rider deflection.	—	·217611	—	·214181	—	·218112	—	·223147

TABLE III.—(*continued*).

	i. (33)	*r.* (34)	*i.* (35)	*m.* (36)	*i.* (37)	*r.* (38)	*i.* (39)	*m.* (40)
Scale readings. . .	1049 1067 1057 1064	1221 1131 1182 1154	1004 1095 1045 1072	1097 1079 1089 1082	1053 1069 1059 1066	1224 1134 1183 1156	1007 1096 1047 1075	1101 1079 1091 1085
Centre of swing . .	1060·80	1163·75	1062·60	1085·20	1062·95	1165·70	1064·65	1086·90
Deflection due to rider or mass .	—	102·050	—	22·425	—	101·900	—	22·000
Mass deflection ÷ rider deflection.	—	·221583	—	·219907	—	·217983	—	·215898

	i. (41)	*r.* (42)	*i.* (43)	*m.* (44)	*i.* (45)	*r.* (46)	*i.* (47)	*m.* (48)
Scale readings. . .	1054 1072 1061 1068	1226 1135 1185 1157	1009 1096 1048 1076	1101 1081 1093 1086	1054 1073 1064 1068	1225 1135 1187 1159	1008 1099 1048 1080	1102 1081 1094 1089
Centre of swing . .	1065·15	1167·15	1065·35	1088·55	1066·85	1168·40	1067·00	1089·70
Deflection due to rider or mass. .	—	101·90	—	22·450	—	101·475	—	21·625
Mass deflection ÷ rider deflection.	—	·218106	—	·220774	—	·217172	—	·213607

	i. (49)	*r.* (50)	*i.* (51)	*m.* (52)	*i.* (53)	*r.* (54)	*i.* (55)
Scale readings . .	1058 1075 1066 1072	1228 1138 1189 1161	1014 1102 1053 1080	1104 1086 1097 1091	1059 1076 1068 1074	1229 1141 1192 1163	1019 1103 1055 1081
Centre of swing . .	1069·15	1170·80	1070·45	1093·00	1070·95	1173·40	1072·15
Deflection due to rider or mass . .	—	101·000	—	22·300	—	101·850	
Mass deflection ÷ rider deflection. .	—	·217458	—	·219867	—	—	

May 25. Afternoon. Mean of 25 determinations of $M/R = a$ } ·21701412.
Attracted masses in upper position }

Mean of 50 determinations, morning and afternoon, ·2157379.

TABLE III.—(*continued*).

SUMMARY OF SET I.

February 4	$a = \cdot2142212$
May 25	$a = \cdot2157379$
Mean value of . .	$a = \cdot2149791$
April 30	$A = 1\cdot010905$
May 4	$A = 1\cdot011032$
Mean value of . .	$A = 1\cdot0109685$

Therefore $A - a = \cdot7959894$.

SET II.

All Attracting and Attracted Masses inverted and changed over, each to the other side. The Suspending Rods also reversed and Riders interchanged. The initial position always the higher reading on the scale.

I. ATTRACTED Masses in Lower Position. July 28, 1890; 8.10 to 9.43 P.M. Temperature: in observing room, 17°–16°·9; in balance room, 15°·4; barometer, 747·6–748 millims. Weather fine and calm; wind W.

	i. (1)	*r.* (2)	*i.* (3)	*m.* (4)	*i.* (5)	*r.* (6)	*i.* (7)	*m.* (8)
Scale readings. . .	1099	912	1130	917	1126	914	1131	922
	1051	1007	1034	1005	1036	1008	1035	1005
	1081	951	1093	952	1091	951	1093	954
	1063	985	1057	985	1058	986	1057	984
Centre of swing . .	1069·65	971·95	1070·55	972·10	1070 20	972·60	1070·95	973·15
Deflection due to rider or mass .	—	98·150	—	98·275	—	97·975	—	97·575
Mass deflection ÷ rider deflection.	—	—	—	1·00217	—	·99949	—	·99541

TABLE III.—(*continued*).

	i. (9)	*r.* (10)	*i.* (11)	*m.* (12)	*i.* (13)	*r.* (14)	*i.* (15)	*m.* (16)
Scale readings. . .	1128	913	1134	924	1130	915	1137	919
	1035	1010	1035	1006	1038	1013	1034	1012
	1092	951	1095	956	1094	953	1098	955
	1058	987	1058	987	1061	989	1060	989
Centre of swing . .	1070·50	972·30	1072·25	975·00	1073·05	975·65	1073·80	976·40
Deflection due to rider or mass .	—	98·075	—	97·650	—	97·775	—	97·700
Mass deflection ÷ rider deflection.	—	·99528	—	·99719	—	·99898	—	·99719

	i. (17)	*r.* (18)	*i.* (19)	*m.* (20)	*i.* (21)	*r.* (22)	*i.* (23)	*m.* (24)
Scale readings. . .	1132	917	1140	924	1134	916	1136	960
	1040	1014	1036	1009	1042	1016	1041	991
	1095	954	1099	958	1098	955	1098	972
	1062	989	1060	990	1064	993	1064	983
Centre of swing . .	1074·40	976·55	1075·05	977·40	1076·90	978·20	1076·70	979·05
Deflection due to rider or mass .	—	98·175	—	98·575	—	98·600	—	98·100
Mass deflection ÷ rider deflection.	—	·99962	—	1·00191	—	·99734	—	·99506

	i. (25)	*r.* (26)	*i.* (27)	*m.* (28)	*i.* (29)	*r.* (30)	*i.* (31)	*m.* (32)
Scale readings. . .	1133	916	1142	925	1134	918	1143	925
	1044	1018	1039	1013	1045	1019	1042	1018
	1098	956	1103	960	1101	957	1103	961
	1065	994	1064	994	1068	997	1066	996
Centre of swing . .	1077·60	979·55	1078·65	980·30	1079·80	981·00	1080·00	982·65
Deflection due to rider or mass .	—	98·575	—	98·925	—	98·900	—	97·075
Mass deflection ÷ rider deflection.	—	·99937	—	1·00190	—	·99090	—	·99031

TABLE III.—(*continued*).

	i. (33)	*r.* (34)	*i.* (35)	*m.* (36)	*i.* (37)	*r.* (38)	*i.* (39)	*m.* (40)
Scale readings. . .	1136 1046 1099 1068	924 1019 961 996	1145 1042 1104 1067	930 1016 964 995	1140 1046 1104 1069	918 1022 959 997	1143 1045 1104 1068	928 1018 962 996
Centre of swing . .	1079·45	982·95	1080·75	983·50	1082·00	982·80	1081·80	983·30
Deflection due to rider or mass .	—	97·150	—	97·875	—	99·100	—	98·875
Mass deflection ÷ rider deflection.	—	1·00335	—	·99745	—	·99268	—	1·00051

	i. (41)	*r.* (42)	*i.* (43)	*m.* (44)	*i.* (45)	*r.* (46)	*i.* (47)	*m.* (48)
Scale readings. . .	1138 1047 1104 1071	926 1020 963 997	1146 1045 1107 1069	928 1022 964 998	1142 1047 1106 1071	927 1021 963 999	1144 1047 1107 1070	937 1015 967 996
Centre of swing . .	1082·55	984·45	1083·45	985·75	1083·70	985·10	1084·05	985·10
Deflection due to rider or mass .	—	98·550	—	97·825	—	98·775	—	98·900
Mass deflection ÷ rider deflection.	—	·99797	—	·99151	—	·99582	—	1·00139

	i. (49)	*r.* (50)	*i.* (51)	*m.* (52)	*i.* (53)	*r.* (54)	*i.* (55)
Scale readings . .	1140 1049 1105 1072	923 1024 962 999	1148 1045 1108 1071	932 1021 966 998	1141 1050 1106 1072	924 1024 963 1001	1144 1048 1107 1072
Centre of swing . .	1083·95	985·40	1084·35	986·60	1084·80	986·25	1084·75
Deflection due to rider or mass . .	—	98·750	—	97·975	—	98·525	
Mass deflection ÷ rider deflection. .	—	·99684	—	·99328	—	—	

July 28, 1890. Mean of 25 determinations of M/R = A } ·9973168.
Attracted masses in lower position }

TABLE III.—(*continued*).

September 17, 1890; 8.0 to 9.31 P.M. Temperature: in observing room, 17°–17°·5; in balance room, 15°·8; barometer, 746·2–746·4 millims. Weather warm, cloudy.

	i. (1)	*r.* (2)	*i.* (3)	*m.* (4)	*i.* (5)	*r.* (6)	*i.* (7)	*m.* (8)
Scale readings. . .	1085	908	1118	921	1109	905	1126	921
	1051	1004	1029	995	1036	1006	1026	996
	1073	945	1085	949	1081	944	1087	951
	1058	981	1050	978	1053	981	1050	978
Centre of swing . .	1064·20	967·35	1063·40	966·70	1063·75	967·35	1063·95	967·90
Deflection due to rider or mass .	—	96·450	—	96·875	—	96·500	—	96·450
Mass deflection ÷ rider deflection .	—	—	—	1·00415	—	1·00168	—	·99613

	i. (9)	*r.* (10)	*i.* (11)	*m.* (12)	*i.* (13)	*r.* (14)	*i.* (15)	*m.* (16)
Scale readings. . .	1113	907	1126	929	1110	910	1131	934
	1034	1006	1027	993	1038	1007	1027	993
	1084	944	1088	953	1083	947	1092	956
	1053	982	1052	978	1056	984	1052	979
Centre of swing .	1064·75	967·75	1065·05	968·40	1065·90	969·90	1067·10	970·20
Deflection due to rider or mass .	—	97·150	—	97·075	—	96·600	—	96·850
Mass deflection ÷ rider deflection .	—	·99601	—	1·00206	—	1·00375	—	1·00026

	i. (17)	*r.* (18)	*i.* (19)	*m.* (20)	*i.* (21)	*r.* (22)	*i.* (23)	*m.* (24)
Scale readings. . .	1104	910	1129	924	1116	909	1121	927
	1044	1008	1030	1001	1040	1009	1036	1000
	1081	947	1091	953	1086	947	1088	956
	1059	985	1054	983	1057	986	1056	984
Centre of swing . .	1067·00	970·44	1067·90	971·45	1066·70	970·85	1067·05	972·80
Deflection due to rider or mass .	—	97·050	—	958·50	—	96·025	—	95·550
Mass deflection ÷ rider deflection .	—	·99279	—	·99288	—	·99662	—	·99105

TABLE III.—(*continued*).

	i. (25)	*r.* (26)	*i.* (27)	*m.* (28)	*i.* (29)	*r.* (30)	*i.* (31)	*m.* (32)
Scale readings . . .	1112 1043 1086 1060	914 1009 951 987	1131 1033 1093 1056	929 1002 957 984	1114 1044 1087 1061	916 1011 952 988	1132 1035 1094 1058	934 1002 960 986
Centre of swing . .	1069·65	973·10	1070·15	974·00	1070·70	974·50	1071·70	976·00
Deflection due to rider or mass .	—	96·800	—	96·425	—	96·700	—	96·550
Mass deflection÷rider deflection .	—	·99161	—	·99664	—	·99780	—	·99690

	i. (33)	*r.* (34)	*i.* (35)	*m.* (36)	*i.* (37)	*r.* (38)	*i.* (39)	*m.* (40)
Scale readings. . .	1097 1058 1083 1067	916 1015 954 991	1135 1037 1098 1061	942 999 965 986	1119 1048 1093 1066	919 1017 957 993	1136 1039 1099 1063	935 1006 963 989
Centre of swing . .	1073·40	977·10	1074·80	977·85	1075·80	979·70	1076·25	979·10
Deflection due to rider or mass .	—	97·000	—	97·450	—	96·325	—	97·025
Mass deflection÷rider deflection.	—	1·00000	—	1·00815	—	1·00947	—	1·00362

	i. (41)	*r.* (42)	*i.* (43)	*m.* (44)	*i.* (45)	*r.* (46)	*i.* (47) .	*m.* (48)
Scale readings. . .	1122 1048 1093 1065	917 1018 956 994	1141 1038 1101 1062	929 1011 962 993	1118 1053 1093 1068	921 1019 958 996	1141 1041 1103 1065	941 1009 966 993
Centre of swing . .	1076·00	979·45	1076·95	980·65	1078·20	981·40	1079·40	982·65
Deflection due to rider or mass .	—	97·025	—	96·925	—	97·400	—	96·975
Mass deflection÷rider deflection .	—	·99948	—	·99704	—	·99538	—	·99628

TABLE III.—(*continued*).

	i. (49)	*r.* (50)	*i.* (51)	*m.* (52)	*i.* (53)	*r.* (54)	*i.* (55)
Scale readings . .	1134 1047 1100 1067	920 1022 958 998	1143 1041 1104 1065	932 1016 964 996	1134 1048 1102 1068	925 1021 962 997	1140 1045 1104 1068
Centre of swing . .	1079·85	982·65	1080·00	983·85	1081·15	984·20	1081·55
Deflection due to rider or mass . .	—	97·275	—	96·725	—	97·150	
Mass deflection ÷ rider deflection. .	—	·99563	—	·99499	—	—	

September 1890. Mean of 25 determinations of M/R = A, Attracted masses in lower position } ·9984148.

July 28 and September 17 } Mean of 50 determinations of M/R = A, ·9978658.

II. ATTRACTED Masses in Upper Position. September 23, 1890; 7.52 to 9·30 P.M. Temperature: in observing room, 15°·3–15°·4; in balance room, 15°·05. Barometer, 749·8–750·2 millims. Weather, light S.W. wind and clear after heavy showers. Scale readings between about 1100 and 1300; 1000 omitted.

	i. (1)	*r.* (2)	*i.* (3)	*m.* (4)	*i.* (5)	*r.* (6)	*i.* (7)	*m.* (8)
Scale readings . .	307 248 285 263	113 210 151 186	329 235 293 257	235 257 243 251	281 261 273 265	112 208 149 185	326 232 290 256	233 256 241 249
Centre of swing . .	271·00	173·10	270·95	248·25	268·35	171·45	268·25	246·60
Deflection due to rider or mass . .	—	97·875	—	21·400	—	96·850	—	21·175
Mass deflection ÷ rider deflection. .	—	—	—	·219797	—	·219799	—	·218581

	i. (9)	*r.* (10)	*i.* (11)	*m.* (12)	*i.* (13)	*r.* (14)	*i.* (15)	*m.* (16)
Scale readings . .	279 260 272 264	110 207 148 183	331 228 290 253	232 255 239 248	277 258 271 262	110 205 147 182	324 229 288 252	230 254 239 247
Centre of swing .	267·30	170·15	266·80	245·15	265·80	168·90	265·55	244·50
Deflection due to rider or mass . .	—	96·900	—	21·150	—	96·775	—	20·225
Mass deflection ÷ rider deflection .	—	·218395	—	·218407	—	·213769	—	·209179

TABLE III.—(*continued*).

	i. (17)	*r.* (18)	*i.* (19)	*m.* (20)	*i.* (21)	*r.* (22)	*i.* (23)	*m.* (24)
Scale readings . .	275	108	323	228	276	107	328	226
	256	204	228	253	255	203	224	252
	269	145	286	237	268	145	287	236
	261	181	251	247	260	179	249	245
Centre of swing .	263·90	167·40	264·10	243·15	263·00	166·65	263·25	241·85
Deflection due to rider or mass . .	—	96·600	—	20·400	—	96·475	—	20·225
Mass deflection ÷ rider deflection .	—	·210274	—	·211317	—	·210547	—	·209652

	i. (25)	*r.* (26)	*i.* (27)	*m.* (28)	*i.* (29)	*r.* (30)	*i.* (31)	*m.* (32)
Scale readings . .	274	106	320	232	271	100	317	222
	254	199	224	245	252	197	221	247
	265	143	283	237	262	138	281	232
	258	176	246	241	255	175	243	241
Centre of swing .	260·90	163·90	260·40	239·85	258·25	160·50	257·90	237·60
Deflection due to rider or mass . .	—	96·750	—	19·475	—	97·575	—	19·100
Mass deflection ÷ rider deflection .	—	·205323	—	·200437	—	·197668	—	·196730

	i. (33)	*r.* (34)	*i.* (35)	*m.* (36)	*i.* (37)	*r.* (38)	*i.* (39)	*m.* (40)
Scale readings . .	266	98	317	221	265	97	311	218
	249	197	219	244	246	193	218	242
	259	136	279	228	257	135	275	226
	254	173	241	237	251	171	240	235
Centre of swing . .	255·50	159·10	255·90	234·20	253·05	157·00	253·30	232·10
Deflection due to rider or mass . .	—	96·600	—	20·275	—	96·175	—	20·150
Mass deflection ÷ rider deflection . .	—	·203804	—	·210351	—	·210164	—	·209271

	i. (41)	*r.* (42)	*i.* (43)	*m.* (44)	*i.* (45)	*r.* (46)	*i.* (47)	*m.* (48)
Scale readings . .	264	95	310	215	261	91	311	212
	243	191	216	239	241	188	210	236
	256	133	273	223	253	128	272	220
	249	168	238	232	246	166	234	229
Centre of swing . .	251·20	154·90	251·40	229·10	248·55	151·10	248·48	226·10
Deflection due to rider or mass . .	—	96·400	—	20·875	—	97·375	—	20·675
Mass deflection ÷ rider deflection . .	—	·212785	—	·215456	—	·213350	—	·213585

TABLE III.—(*continued*).

	i. (49)	*r.* (50)	*i.* (51)	*m.* (52)	*i.* (53)	*r.* (54)	*i.* (55)	*m.* (56)
Scale readings . .	257	90	306	211	256	88	303	208
	237	186	208	234	236	184	209	232
	250	127	269	218	248	125	265	216
	243	162	232	227	242	160	231	225
Centre of swing . .	245·15	149·25	245·80	224·10	244·25	147·21	243·95	222·10
Deflection due to rider or mass . .	—	96·225	—	20·925	—	96·900	—	20·350
Mass deflection ÷ rider deflection . .	—	·216160	—	·216699	—	·212977	—	·209956

	i. (57)	*r.* (58)	*i.* (59)	*m.* (60)	*i.* (61)
Scale readings	253	86	301	203	293
	233	180	205	228	204
	246	122	263	213	280*
	238	157	227	222	245
Centre of swing	240·95	144·00	240·95		
Deflection due to rider or mass . . .	—	96·950			

September 23, 1890. Mean of 27 determinations of $M/R = a$ } ·2112753.
Attracted masses in upper position }

September 25, 1890; 7.10 to 8.43 P.M. Temperature: in observing room, 15°–15°·2; in balance room, 15°. Barometer, 760·8, steady. Weather cloudy, with westerly airs. Time of swing 21 seconds. 1000 omitted in scale readings.

	i. (1)	*r.* (2)	*i.* (3)	*m.* (4)	*i.* (5)	*r.* (6)	*i.* (7)	*m.* (8)
Scale readings . .	246	84	301	206	248	82	297	202
	238	179	205	229	233	178	204	228
	243	121	263	215	243	119	260	212
	239	156	228	224	236	156	226	222
Centre of swing . .	240·90	142·90	240·95	220·40	238·95	141·60	238·95	218·10
Deflection due to rider or mass . .	—	98·025	—	19·550	—	97·350	—	20·850
Mass deflection ÷ rider deflection .	—	—	—	·200128	—	·207499	—	·213163

* This is a considerable rise, showing either a sudden disturbance or a displacement of the apparatus; possibly the telescope was touched. The rise was maintained and therefore the observations were discontinued.

TABLE III.—(*continued*).

	i. (9)	*r.* (10)	*i.* (11)	*m.* (12)	*i.* (13)	*r.* (14)	*i.* (15)	*m.* (16)
Scale readings . .	248	83	300	204	252	84	303	207
	233	176	203	228	233	182	206	232
	243	119	261	214	245	122	265	217
	236	155	226	224	239	158	228	226
Centre of swing . .	238·95	140·80	239·20	219·50	240·70	144·60	242·45	222·60
Deflection due to rider or mass . .	—	98·275	—	20·450	—	96·975	—	20·825
Mass deflection ÷ rider deflection .	—	·210125	—	·209475	—	·212813	—	·214718

	i. (17)	*r.* (18)	*i.* (19)	*m.* (20)	*i.* (21)	*r.* (22)	*i.* (23)	*m.* (24)
Scale readings . .	255	87	307	210	257	90	271	215
	237	184	207	234	238	184	233	233
	249	125	267	219	251	127	255	222
	242	162	232	228	244	163	241	229
Centre of swing . .	244·40	147·55	244·70	224·65	246·05	148·75	246·70	226·15
Deflection due to rider or mass . .	—	97·000	—	20·725	—	97·625	—	20·725
Mass deflection ÷ rider deflection. .	—	·214175	—	·212974	—	·212292	—	·212564

	i. (25)	*r.* (26)	*i.* (27)	*m.* (28)	*i.* (29)	*r.* (30)	*i.* (31)	*m.* (32)
Scale readings . .	258	90	307	213	262	93	307	215
	241	186	213	237	242	189	215	239
	251	129	270	223	253	131	272	225
	244	164	236	232	246	167	237	233
Centre of swing . .	247·05	150·45	248·60	228·30	248·85	153·00	250·25	230·05
Deflection due to rider or mass . .	—	97·375	—	20·425	—	96·550	—	19·750
Mass deflection ÷ rider deflection. .	—	·211297	—	·210649	—	·208053	—	·204425

	i. (33)	*r.* (34)	*i.* (35)	*m.* (36)	*i.* (37)	*r.* (38)	*i.* (39)	*m.* (40)
Scale readings . .	261	93	312	215	263	96	312	217
	243	189	214	241	245	192	217	243
	253	132	273	225	257	135	275	227
	247	167	237	235	250	168	240	237
Centre of swing .	249·35	153·40	250·80	231·10	252·40	156·05	253·05	233·05
Deflection due to rider or mass . .	—	96·675	—	20·500	—	96·675	—	20·525
Mass deflection ÷ rider deflection .	—	·208172	—	·212051	—	·212180	—	·212254

TABLE III.—(*continued*).

	i. (41)	*r.* (42)	*i.* (43)	*m.* (44)	*i.* (45)	*r.* (46)	*i.* (47)	*m.* (48)
Scale readings . .	264	98	314	220	267	100·	321	224
	247	194	219	243	249	197	223	246
	259	136	277	231	260	138	281	233
	251	171	242	238	255	174	246	242
Centre of swing .	254·10	157·85	255·05	235·20	256·25	160·35·	259·30	238·05
Deflection due to rider or mass . .	—	96·725	—	20·450	—	97·425	—	21·250
Mass deflection ÷ rider deflection .	—	·211812	—	·210662	—	·214011	—	·218650

	i. (49)	*r.* (50)	*i.* (51)	*m.* (52)	*i.* (53)	*r.* (54)	*i.* (55)
Scale readings	271	102	321	224	271	102	314
	252	200	221	247	251	198	226
	264	139	282	233	264	140	280
	256	176	245	242	256	174	247
Centre of swing	259·30	162·20	259·00	238·40	258·95	161·60	259·50
Deflection due to rider or mass	—	96·950	—	20·575	—	97·625	—
Mass deflection ÷ rider deflection	—	·215704	—	·211487			

September 25, 1890. Mean of 25 determinations of $M/R = a$ (Attracted masses in upper position) ·21125332.

September 23 and September 25 Mean of 52 determinations of $M/R = a$, ·2112647.

SUMMARY OF SET II.

July 28	$A = ·9973168$
September 17 . . .	$A = ·9984148$
Mean value of . .	$A = ·9978658$
September 23 . . .	$a = ·2112753$
September 25 . . .	$a = ·2112533$
Mean value of . .	$a = ·2112647$

Therefore $A - a = ·7866011$.

Mean value, giving equal weights to Sets I. and II.,

$A - a = ·791295$.

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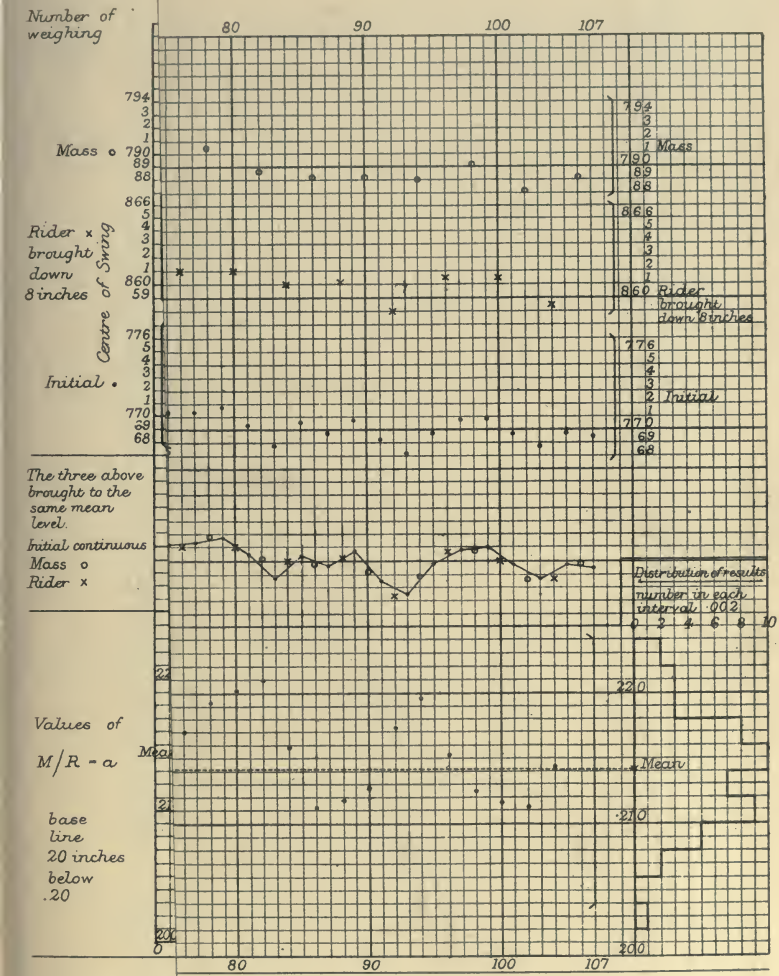

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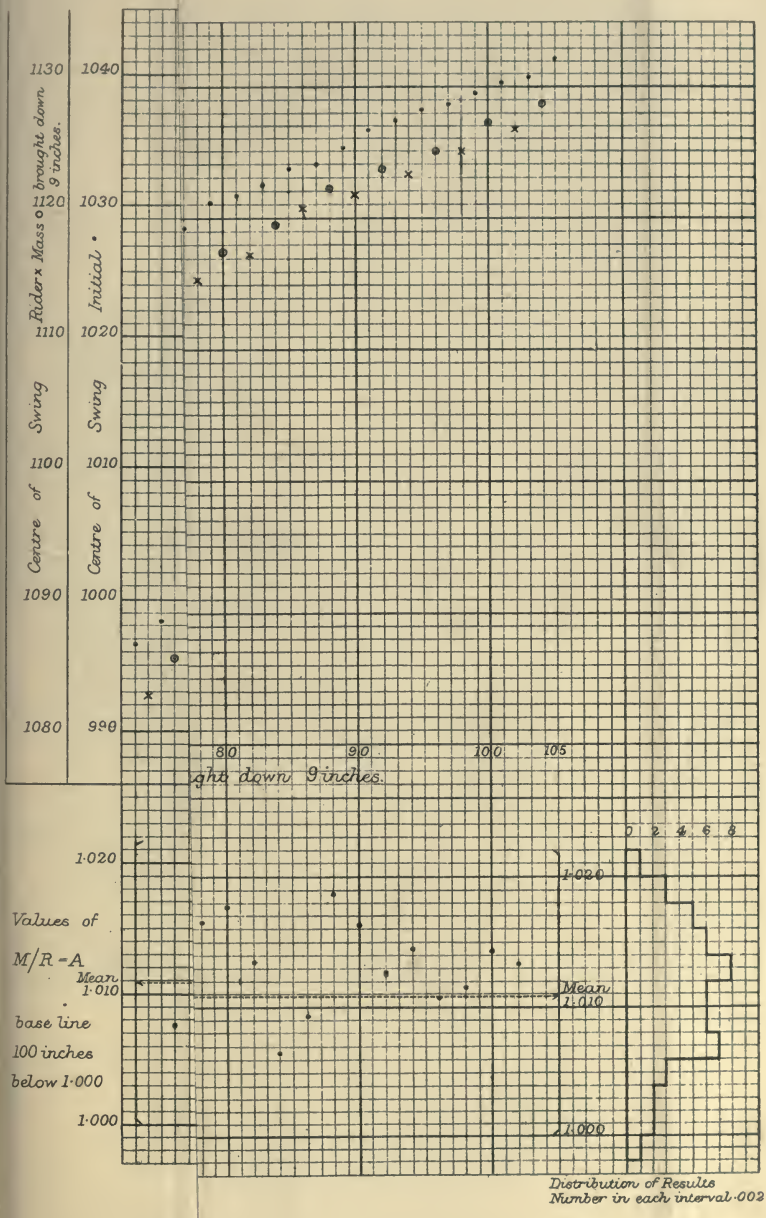

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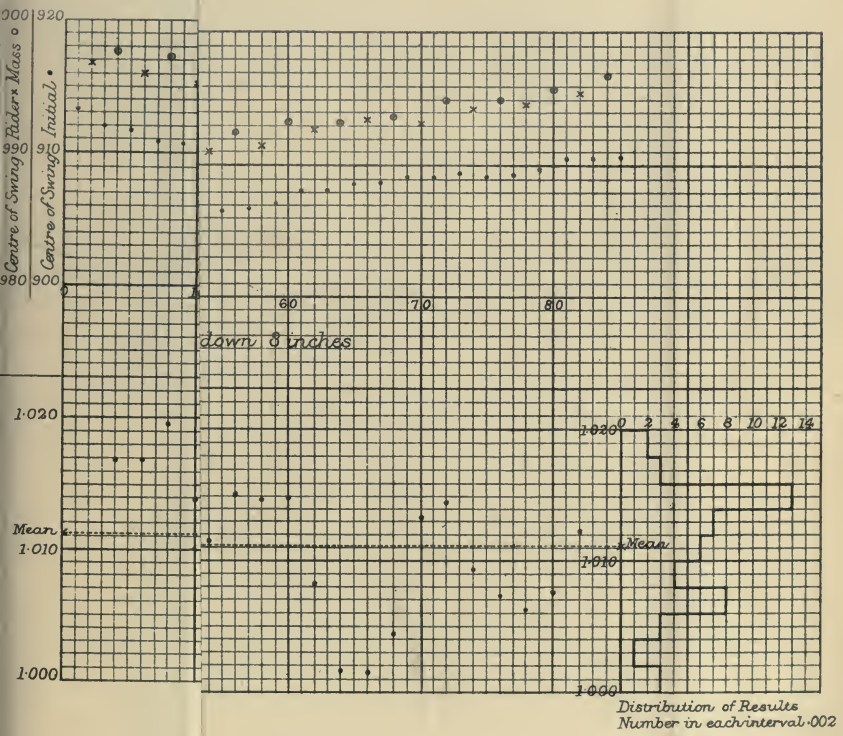

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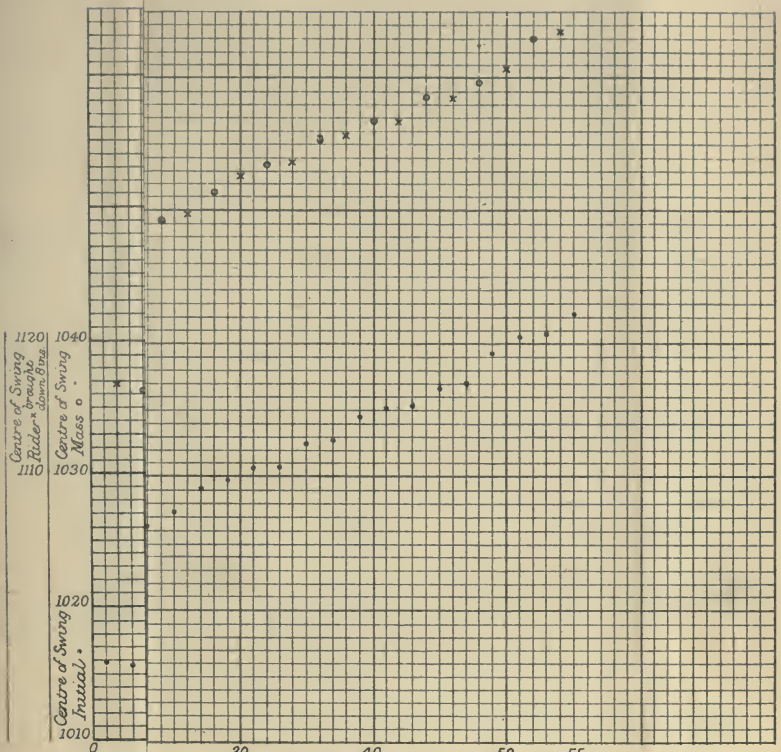

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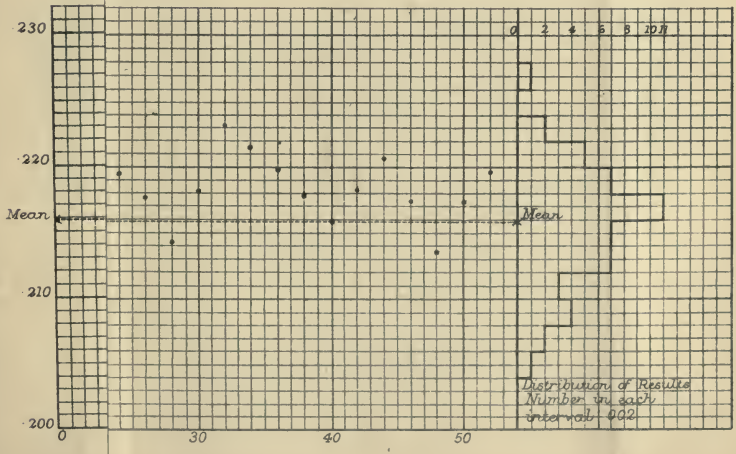

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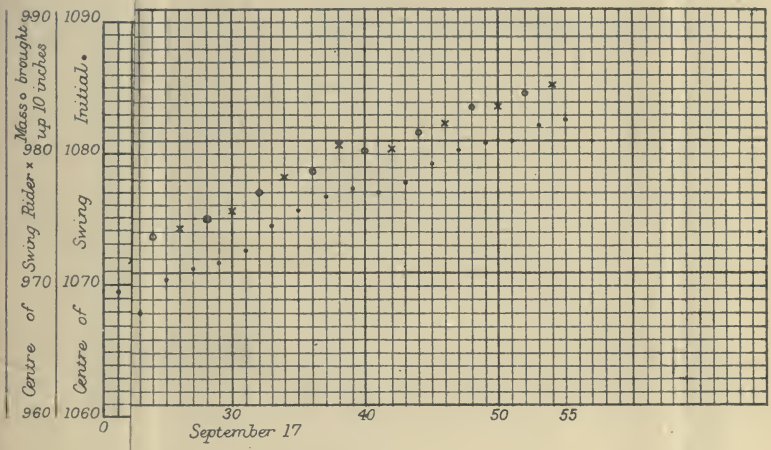

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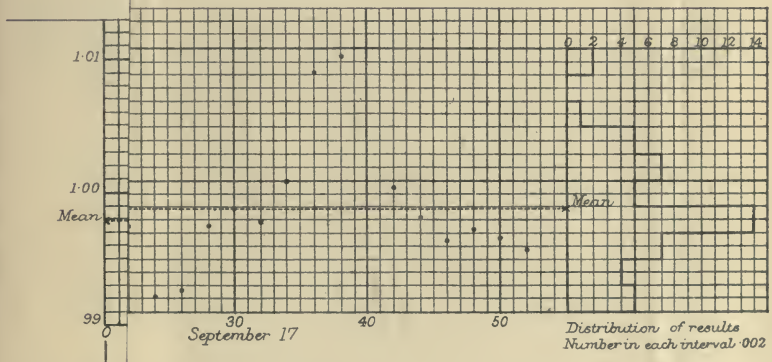

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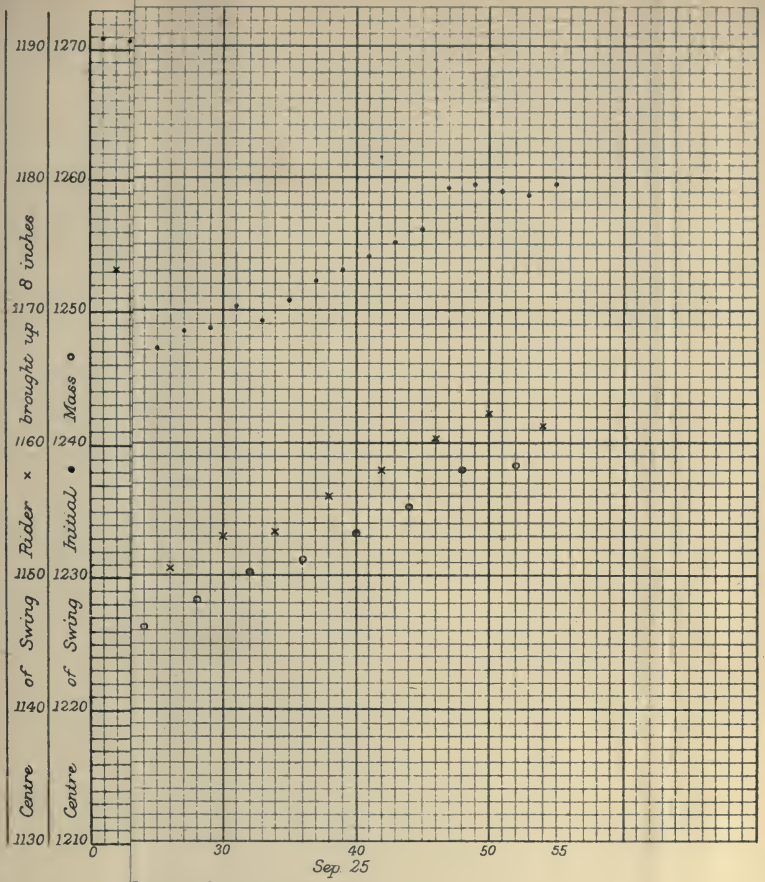

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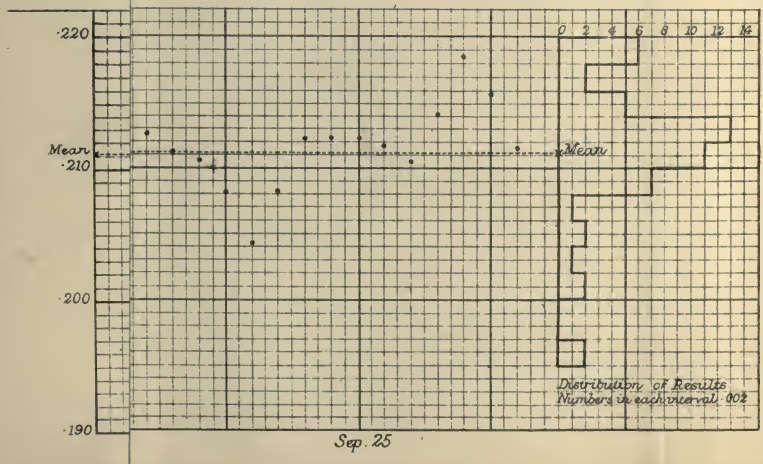

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